

# Psychological Bulletin

## PSYCHOLOGICAL FACTS AND PSYCHOLOGICAL THEORY\*

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A fact has a peculiar and intricate structure. It belongs to two worlds, the world of objects and events, and the world of human discourse. Facts are invisible and inaudible. They can not be burned in furnaces or used to roof houses, or shot from big guns.

Objects and events are not facts; they are merely objects and events. They are not facts until they are described by persons. And it is in the nature of that description that the quintessence of fact lies. Only when an event has been given a very specific kind of description does it become a fact.

When we say, "Let's get down to the facts," what we are saying is much more than that we should look at or listen to or smell or touch real objects, or that we should all observe an event. What we are really proposing is that we all try to find certain statements on which we can all agree. Facts are the basis of human cooperation.

*A fact is an event so described that any observer will agree to the description.* There are, of course, no facts that meet this too general requirement. We are satisfied—we have established our fact—if any observer within the circle of persons with whom we discuss events will agree. There are always feeble-minded persons, ignorant persons, insane persons, apathetic persons, whom we disregard. There are, therefore, no absolute facts, and a universe without men and human discourse would be a universe without facts.

It may be readily granted that this definition of a fact is no fact itself. There are many men who would hold that facts are just events and objects, and will so continue to assert even after they hear this definition. The definition is made, however, with the hope that certain elite hearers will immediately accept it. They are the audience to whom the definition is addressed.

Psychological facts are events so described that any psychologist

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will accept the description. This description is done perforce by psychologists and for the benefit of other psychologists. Laymen will not necessarily be familiar with the language itself, or with what should be looked at (for, after all, facts are based upon what is seen and heard and are not pure inventions of men.)

Facts are of particular importance to science. Science is founded on an interest in fact. By that I do not mean an interest in nature. I mean an interest in getting down to a factual basis which starts with descriptions acceptable to any observer. This agreement must not be limited to the members of a cult or the partisans of a cause, or the speakers of a language. Sun worshippers may all agree that the Sun God has reappeared with the new day. Members of other cults will not accept his divine attributes. But all men in any language will agree that the circular disc of light is again present.

The agreement that is essential to facts can not depend upon skill or judgment or taste, unless we have assurance that that skill or judgment or taste can readily be acquired with training. The expert taster, the connoisseur of pictures, the skilled diagnostician does not contribute to the advance of science unless he can discover a factual basis for his judgments open to all interested men.

As psychologists we face in the near future the onrush of a torrent of new facts. The number of psychologists has doubled in a few years and if we read the signs correctly is about to double again in an even shorter period. Even the present members of the Association will be contributing to the deluge if we can interpret the fact that during one year approximately three-fourths of us (three thousand of us in round numbers) have changed our addresses and presumably have encountered new persons and problems and new scenes. Industrial psychologists, school psychologists, personnel psychologists, clinical psychologists, will soon be filling the pages of our journals with statements which will, it is to be hoped, include the right proportion of facts. We hope to remain on a factual basis.

But that is not to be taken for granted. A flood of new publications is not automatically a flood of new facts. And it may include many facts which do not contribute materially to the science of psychology. Collections of facts are not science. They are the material out of which science can grow, but they are only the raw material of science, and sometimes they are not even that.

Psychological facts are events described in psychological terms and therefore by and for psychologists. The descriptions which facts require have not been lying about waiting to be noticed. They are the result of hard work and careful and devoted attention. And their value depends

on the insight and good judgment of their collectors. There are useless and misleading facts as well as useful and enlightening facts. There can be a great wastage of paper and of human effort in the publication of facts. Collections of facts for their own sake are of no more value than the collections of old objects.

Some facts are useful in themselves. Medical knowledge includes many facts about the effects of drugs and treatment for which there is no rationale, and as psychology extends into new fields it will welcome many facts about human behavior that cannot be fitted into any theory. The success of applied psychology will depend on the accumulation of much knowledge of this sort. And knowledge of this sort must be taught to students if they are to become practitioners. The open-mindedness of the physician toward facts of this sort which have no bearing on theories has saved the lives of many of us and is the real mark of the physician as distinguished from the scientist. The physician is interested in the cure of his patients. His success depends on his acquaintance with thousands of medical facts. The discovery of penicillin may lead to a saving of human life years that will match the loss of the fifty million lives in the war just finished. But until many researchers have patiently collected the relevant scientific facts that enable them to make scientific generalizations about how penicillin brings about its results, the discovery of its healing effect is not yet a contribution to the sciences of medicine and physiology. It is only a contribution to the tools of the physician.

My own personal bias in psychology is toward an understanding of learning and habit formation. In the two fields of learning and of motivation will be worked out the basic theory that will eventually make the science of psychology a much more powerful instrument than it now is. When we are able to state the general principles which govern human learning we shall have the most important tool needed for the prediction and control of human behavior.

Nothing is more familiar to men than human learning and habit formation. Men have lived intimately with the phenomenon since long before they reached the human state, and they have in myriad ways remarked and described it. But a scientific theory of learning has yet to be agreed upon by psychologists. Such a theory is essential to progress for several reasons. One of these is that unless the beads of fact can be strung in order and pattern on the threads of a theory, there is a strict limitation upon imparting psychological knowledge to others. Theories are mnemonic devices that make science teachable. And theories are the basis of working concepts. They enable men to confront new facts and deal with them successfully. Furthermore, theories are

required to direct the search for relevant facts. It is theories that endure, not facts. Events are ephemeral and their descriptions also may be ephemeral. It is theory that lasts for years or for generations. It is theory rather than fact that leads to new controls over nature and events. From theory inferences can be made and new applications devised. Facts are likely to be local and temporary. Their applications are limited.

A blacksmith may have collected much skill at his trade, and a few facts. He has learned that iron bought from a certain firm has certain good qualities. He can, by watching the color, judge the moment for making his weld. He knows how to temper his steel and how to draw its temper. The science of metallurgy goes in for a very different collection of facts. The science of metallurgy lies in making a different collection of facts. It is not interested in subjective color but in temperature. It substitutes chemical analysis for facts about market source. Market sources are ephemeral and smiths report colors less reliably than thermometers and thermocouples report temperature.

Like metallurgy, a scientific psychology consists in a new orientation toward psychological facts, a weeding out of subjective descriptions and an avoidance of descriptions colored by values and prejudices that are not universally shared. At the present moment our science is entirely too tolerant of such concepts as adjustment, reward and punishment, success and failure. These are all strongly flavored by values that are non factual. The approval of our own group or cult determines what we shall call an adjustment or what we shall call success.

It is my own conviction that in the field of learning the great majority of studies have been collecting unpromising kinds of facts. They have collected facts analogous to the blacksmith's lore concerning how long a particular tempering of his iron will wear upon the horse's hoof, how well pleased his patrons are with his wagon tires, how fatigued he will be with one method of welding as compared with another.

The reason for this is that we have allowed ourselves to be too much influenced by the desire for results of immediate practical application. This has led to the common acceptance by psychologists of a definition of learning in terms of practical value. Most psychologists, when they use the word *learning*, mean the acquisition of socially approved modes of behavior, improvement in performance, in economy of effort and of time in attaining conventional goals. The early writers on learning, Thorndike, Lloyd Morgan, Hobhouse, defined learning in terms of achievement. The animal learns a task set for him by the experimenter. He improves his accomplishment.

This conception, of course, is in good accord with practical common



sense. It is what gets done that is of practical importance, not the response of the person, but the results of that response. But to use practical achievement, goal attainment, success, as the essential criterion of learning, and to turn our search for facts to the observation of success and the conditions under which it is attained is analogous to the use of money value by the chemist as his chief descriptive term in observing a chemical reaction, or the definition by the physicist of work in terms of useful work or valuable work. All the psychologies which are written in terms of "least effort" or of goal achievement are by that choice rejecting the possibility of developing an objective and scientific psychology. They are, of course, following public interest which is turned toward securing quick results in training, or toward the abolishment of obnoxious habits, the acquisition of paying skills. We shall never learn how skills are acquired if we confine our attention to "improvement" in behavior and use as the criterion of learning the elimination of bad behavior and the acquisition of good, or the accomplishment of praiseworthy results. We must understand the processes through which behavior is changed, whether for better or for worse. A clinical psychologist is properly interested in the "cure" of an enuresis. If he is a real psychologist as well as a clinician, he will be interested in just what alteration in behavior was brought about; the fact that the alteration was acceptable to his patient's family may contribute to his income, but not to his science.

The conception of learning in terms of socially valuable outcomes of action led to the collection of learning curves which indicated the reduction of time and of waste motion with practice. It even led to a perversion of Pavlov's conditioning experiments in which as many as 1500 pairings of stimuli are recorded along with the resulting change in certainty or intensity of response. During that series the phenomenon of learning has occurred at each pairing. The massed effect of the 1500 trials may totally obscure or totally miss what happens at each trial. Studies with the maze, the puzzle box, the acquisition of skills, all record some end-result, but do not collect facts involving the animal itself.

The literature of mental tests had over twenty years ago collected some ten thousand titles and the number must be at least three times that figure by now. For the most part the testers have limited their collection of facts to the marks put on paper by persons being tested, and to the association of these marks with some criterion. They have not examined the behavior of the child taking the test, nor has this enormous literature advanced our understanding of what goes on in a child who marks the third of four possible choices. In other words, the

testing movement has been absorbed in highly useful and practical work, but it has not contributed to psychological theory. It has not advanced our knowledge of how the child's mind works.

In the same way studies in learning have been dominated by practical considerations and their facts collected center in practical outcomes of behavior rather than in the behavior process itself. We must undertake to examine the nature of changes in behavior before we shall have a proper understanding of success and failure.

My first suggestion for directing our attention toward facts that will lead to the development of good theory applies chiefly to the field of learning. It is that we look for facts in the behavior of the organism rather than in the operation of a latch, an arrival at a goal, the "learning" of a lesson. We should transfer our interest from the goal achievement to the behaving organism. It is the muscles of the organism that are innervated, and not the lever of the problem box. The machinery through which solutions are arrived at is contained within the skin of the solver.

May I illustrate what here is meant. Studies of maze learning have kept records of the time and number of errors required on successive trials to get the animal to a particular area. Learning curves have been plotted and learning assumed to be a direct function of the number of trials. Practically no experimenter has taken account of the fact that each animal may radically alter its behavior on successive trials, or that the alteration may have been evident only between the eleventh and the twelfth trials and exhibited no curve at all. The curve is only the resultant of many cumulative learnings, which may have included a number of "unlearnings" as well. The picture of learning as a function of the number of trials may be totally altered when we examine behavior at each choice point separately.

Dr. George P. Horton and I occupied ourselves two pre-war winters in observing and recording some eight hundred escapes of cats from a puzzle box. One startling result of an examination of our photographic records of the posture of the cat at the moment of release is the discovery that a series of escapes often displays a highly routinized pattern and stereotyped posture which appears at widely separated points in the series. Here is an elaborate series of movements extending over a period of many seconds or even minutes which has not disappeared from the cat's repertoire, although it has not been in evidence in the cat's behavior for many trials. It was not unlearned or forgotten, as is proved by its accurate reproduction.

If we had contented ourselves with a record of the time required to escape, we should have missed the real nature of the learning process.

So far as we can judge, improvement in the sense of time reduction consisted in the gradual elimination of movement routines that left the cat in the box. The successful act itself always appeared suddenly, either in the very first trial or in some subsequent trial. It required no long series of repetitions for its establishment.

It has been suggested that it will be profitable to give more attention to the behaving organism if we are to understand learning. There is a second admonition which might well be taken seriously. This is that we may profitably give more attention to stimuli as the occasions for response. No psychologist has seriously challenged the conception that the normal occasion for muscular contraction, and hence for all that an animal does, is the activation of sensory receptors. There are psychologists, however, who believe that the stimulus-response formula has had its day. For myself, I do not believe that it has been yet properly exploited. It requires that, if we are studying learning, we observe the response actually following stimulation. Many recent experimenters have, instead, followed Pavlov and observed not the sequence of stimulus-response but the conjunction of two stimuli like bell and food, or buzzer and shock, and have not believed it necessary to notice what actual response followed the signal.

Psychologists who think in terms of punishment and reward have almost uniformly neglected to note how the animal at the time responded to the punishment or to the reward, and the role this played in subsequent behavior. The resulting generalization is inevitably an attempt to link the intentions of the experimenter (intentions to reward or punish) with good or bad behavior on the part of the animal. Punishment and reward are, objectively viewed, stimuli acting on the animal's sense organs, and their effect must be mediated through the animal's nervous system and appear in muscular contraction or glandular secretion. Since levers and loops and mazes are not innervated, the operations of these devices are incidental to the actual learning which the living animal performs.

This failure to examine facts in the field of stimulus-response sequence is, of course, a tradition of psychology. Lloyd Morgan, Hobhouse, Thorndike, responsible for our first careful observations of learning, all were interested primarily in success rather than in response, and all speak in terms of "confirming results." It has occurred to none of them to regard these confirming results as possible stimuli, followed by possible response. Hull, who has endeavored to make the concept of reward over into something much more objective and immediate, so far as I can understand leaves the determination of what it is that will serve to confirm or reinforce quite vague. I believe it will be very

profitable to examine his reinforcements as possible stimuli with close attention to their subsequent responses.

There are many fields in psychology in which the injunction to note and formulate facts concerning the stimulus-response sequence might well be followed. Freud, who makes associative learning the foundation of his whole system, has at no point even asked what it is that is associated, or under what circumstances association is effective. A number of psychologists have in recent years insisted in interposing an O for "organism" between S for "stimulus" and R for "response." There can be no objection to this, provided we make a vigorous effort to determine the classes of fact that we agree to include in this O for "organism" and are not content to leave it as O. There can be no doubt that what is intended to be included in this O is often reducible to a dependence of response on interoceptive and proprioceptive stimuli, and O is a symbol for groups of relevant facts that should be noted and recorded rather than given up.

There is another more legitimate excuse for O. Very properly classed as characters of the organism affecting the stimulus-response sequence are the facts of past learning which can be known only through the record of past behavior. We have as yet no way of noting the brain changes that we assume to be the actualities responsible for changed response to stimulation. There are also legitimately included under O the determiners of behavior sought out by tests, which may be interpreted as behavior samples and assumed to be prognostic of response for varying periods of time. O may also include the material being offered through the more objective methods of examination and interview, and, by inference, the information furnished through the history of the individual.

In all these classes of fact it is of first importance to remember that facts are events so described that any competent observer will accept the description. We should recall that acceptance within limited groups, like the staff of a hospital working under an aggressive leader, or any department under an aggressive chief, may exhibit acceptance on grounds that do not insure that intelligent and informed outsiders will be able to agree to the asserted facts. Stimuli and movements are relatively objective and the agreement necessary to the establishment of fact is relatively easy to obtain. Attitudes and the meaning of behavior are less objective and more likely to produce disagreement among observers. Only on a factual level can the foundations of science be laid. Progress toward scientific psychology must be founded on agreed facts and public facts. The psychoanalysts' interpretations of dreams and of



motivation in general are notably remote from the factual basis that must precede the development of a scientific psychology in that field. But that a factual basis is not unattainable in a field so remote from basic theory as psychotherapy is established by Carl Rogers' recent book, *Counseling and Psychotherapy*, in that he has succeeded in reducing his account to descriptions of events which should prove to be acceptable to psychologists of varied interests and varied theoretical background. This he has even achieved in a number of his quantitative generalizations. He illustrates the type of fact collection which, though made with a highly practical aim, may furnish a basis for the theory that must eventually be developed in order to give system and order to our facts. That theory will extend far beyond psychotherapy into many fields of psychology.

My first suggestion concerning the factual basis for learning theory was that we give more attention to the organism itself, and that we recognize that such classes of fact as improvement, success and failure, reward and punishment, are external and incidental features of learning. The mechanism of learning is within the organism. These external features should be examined only in their role as stimuli to sense organs.

My second suggestion was that the promising factual field for observation is the stimulus-response sequence, and that we should meticulously note such sequences. My third suggestion was that part of what some writers insert in that stimulus-response formula, namely the organism, can, with diligence, be examined in terms of interoceptive and proprioceptive stimuli, often observable and often inferable (as in the case of so-called drives like hunger). Much of the rest of O names facts of the organism's past history, from which we infer changed tendencies to reaction. Such of O as is left over in the form of attitudes we must endeavor to place on a basis of public fact and seek for descriptions which are acceptable to all psychologists.

There is a further admonition. This is that we should undertake more consistently and thoroughly to note what I may call response-stimulus sequences, the stimulus changes following upon the responses of the organism. I have already expressed the opinion that learning and motivation represent the two fields most fundamental to an understanding of behavior and thought. Through close attention to stimulus-response sequences we may formulate the rules of learning, the circumstances under which such sequences change. Through close attention to response-stimulus sequences we may solve many of the problems of motivation and the direction of learning.

It is through observation of the effects of response on stimulation

that we may avoid those vague references to drive and motive that have done so much to obscure the understanding of behavior. A tense bladder through reflex paths operates to relax a sphincter muscle, but that relaxation is inhibited through associative learning by numerous situations. When these associative cues are removed or facilitating associative cues are added, the act occurs. The original stimulation is removed. The incident, save for its effects on future behavior through associative learning is for the time being closed. To invent a drive to explain this act is unnecessary as soon as we are familiar with the stimulus-response antecedents. To allow the disappearance of the restlessness that follows sphincter relaxation to force us to speak in terms of a drive that has attained its goal and is now satisfied is unnecessary when we observe the effects of the response on the new stimulus situation, the R-S sequence.

Every response alters the stimulus situation of an animal. Some responses remove the persistent and insistent stimulus that has been responsible for general activation as well as specific action tendencies. Such responses have a profound effect on the behavior following and on the mode of response that will be acquired by the animal through training.

Other responses leave the stimulus goad in action and the effect is to bring new goads into play. In fact the whole direction of behavior is set by the effects of responses on stimuli. The advocates of the law of effect (Thorndike) or the law of reinforcement (Hull) state the foregoing sentence differently. Their version would be: The whole direction of behavior is set by the effects of responses. You will recall that the version here suggested is: The whole direction of behavior is set by the effects of responses on stimuli. Punishment and reward have no effect on behavior as mere rewarders or reinforcers, but only in so far as they stimulate new behavior. We learn to do what punishment and rewards make us do. We do not necessarily learn to do what was rewarded or learn to abstain from what was punished.

In stimulus-responses there is to be found the key to associative learning. In response-stimulus sequences we may discover the motivation and direction of behavior. That we learn is insured by S-R. Stimulus patterns active at the time a response is initiated become inciters of that response. Because inciters of rival responses may also be active, the response does not always occur; but what effect such stimulus patterns contribute is toward the production of the response with which they were last associated.

That we learn is insured by the association of a stimulus with a re-

sponse. Whether that learning is retained depends on what then follows. It depends on the effect of the response on the new stimulus situation. May I illustrate this with an anecdote of animal learning. The anecdote is not factual in that it describes an event witnessed by only one psychologist and he would be too humane to repeat it. But its analogue is very familiar in the puzzle box behavior which George Horton and I have extensively photographed. The anecdote is this: The psychologist in question has a cat which on entering the kitchen before mealtime limps with a very noticeable limp. This limp is not observed in the cat at other times. Its history is that the cat on one occasion entering the kitchen at mealtime had its foot pinched in the swinging door. The cat made a terrific outcry and continued to limp about and put forth noise. After a quick examination to assure himself that no bones were broken, the psychologist offered the cat its dinner which had been standing ready. Why does the cat persist in limping on later visits to the kitchen?

Horton and I found that every cat we dealt with, between fifty and sixty in all, exhibited very similar behavior. When escape from the puzzle box followed almost any behavior, colliding with the release, pawing it, backing into it, jumping to the top of the box and falling on the release, lying down and inadvertently rolling to contact with the release, heavy odds could be placed that the same movement would be repeated soon after the cat was returned to the box. None of these behaviors had a learning curve. Each appeared suddenly full blown. In only a few cases could anything like an improvement of the successful act be recognized.

It is our belief that this characteristic of learning is explainable in terms of the effect of the response in question on the stimulus situation. Responses which left the cat in the box tended to disappear from its behavior, though in some cases they were very persistent. But the response which opened the escape door was generally preserved. We suggest that this is explainable through the fact that escape removes the cat from the puzzle box but does not allow a new response to be associated with the stimulus situations within the box. The cat has no way to forget. R remains faithful to its association with S because unfaithfulness would require that some rival response become associated with S, but S is now out of the picture. No new associations can be established with an absent stimulus situation.

It is my contention here that we shall gain much new light on behavior if we devote ourselves more zealously to observing the effects of response on stimulation. Every response must have such effects.

Through movement an animal changes its view, the sound pattern affecting its ears, its own pattern of proprioceptive stimulation from muscles and joints. In this radical change which results from action lies the explanation of the direction of our learning.

Our own responses not only bring about changes in the external world. They furnish cues for our further action. They eliminate, or sustain, or produce stimuli to action. And the consequences of this elimination, sustaining, or production are far-reaching.

My dog reaches out and paws my foot as I sit reading. To get my attention, the ordinary observer would say. Of course, the dog does it to get my attention. This is not a fact, however, but an interpretation. Its factual basis is that the dog makes movements or takes a posture that was in the past formed and originated by my attention. Without this factual basis we are speaking on the level of Little Red Riding Hood who is satisfied by the wolf's explanation that his great ears are the better to hear her with.

What would we find the explanation of the dog's gesture if we were to follow the rules that are here suggested and note the history of the event in terms of stimulus-response sequences and response-stimulus sequences? I reject immediately those softer accounts in terms of insight. There must have been a first use of the gesture, and I do not for one moment believe that dogs come into this world equipped with so strange a power for getting results. My own notion of what has happened is that first, I have on a number of occasions scratched the dog behind the ear. The effect of this on the dog is to interrupt all other activities and keep him motionless in place. Common speech uses the word enjoyment; but we might try to stick to psychological facts. The stimulation of my scratching is an essential element of the dog's response. It is what serves to maintain his quiet pose. When my scratching stops, the dog is released, but the sight of me, and the nature of his own response to me serve to call out in him a repetition or prolongation of his "behavior of being scratched." When scratching stops, he is no longer kept quiet by the scratching and is free to move. Whatever movement takes place will be within the limits of his present stance. I do not expect sudden barking or violent action. He has been standing quietly. He may move his head. If his movement had attracted my attention and brought a resumption of the scratching, I should expect that on the next occasion on which the stimulus situation was substantially the same, there would be a repetition of the head movement. This response of the dog's would not be unlearned, because the stimulus situation (waiting unscratched) which had become its cue with one repetition



is gone with the recurrence of the scratching. It is more or less of an accident that his original head movement was unrewarded, and that reward was reserved for a movement of his paw.

I have used the word "reward." That is not a psychological word. My scratching was not effective because it was a reward. It was effective because it prevented the dog from unlearning his gesture with his paw. If I had, instead, cuffed him hard, the cuff would have undone the gesture with the paw as a habit, not because the cuff is a punishment but, in psychological terms, because a hard blow would have established in the dog a tendency to back away from me and there would have been no recurrence of the situation that led to the gesture with the paw.

Once we have let ourselves in for scratching a dog's ears, there is no natural end to the incident save our own fatigue. The dog's response, his quiet posture permitting the scratching, can be continued indefinitely and will be interrupted only by eventual fatigue or an adventitious external event. But there are other actions that are self-terminating. Descending a stair cannot go on indefinitely even though the first steps have established associative serial connections, because one of the peculiarities of staircases is that they have a bottom step and it would be only with the help of a miraculous steamshovel and a corps of engineers that we could be kept provided with steps to descend. If we raise an arm, the arm is now raised and the situation radically changed. The arm can not be again raised until it has first been lowered.

This introduces a new admonition for our fact-collecting. Not only should we note S-R sequences. We should further note that any R in progress, whether that response is active movement or merely the maintenance of a posture, sets strict limits on what can next be done. At any moment there are severe restrictions on the behavior possible to elicit, no matter what new stimuli are offered.

We recall in this connection the work of Magnus on postural reflexes. When a cat has been decerebrated and is stood upon a surface, slight manipulation of its head can result in alterations of its whole muscular set. If the head is turned slightly to the right, the right fore-leg is flexed, the left extended, and the whole posture is made an obvious set for moving to the right. Older members of the profession can recall taking advantage of this postural adjustment by using leathern straps attached to the head of one of the larger animals in order to induce locomotion toward the side on which the rein was pulled.

It is probable that an intact cat in the posture appropriate to taking off toward the right, can not be directly stimulated to take off toward the left. The original posture must be first relinquished and a second taken

on. A whole field here calls for research. What movements are possible from a given stance? What responses are elicitable when a person is maintaining a given attitude? I would here include not only obvious physical attitudes but the more covert states which limit behavior. We must not give up the investigation of attention and readiness.

What are the effects on a going action of sudden irrelevant stimuli? I confess that I do not even know the answer to so simple a problem as what a diner in a restaurant, just raising his cup of coffee to his lips, will do if a police whistle is suddenly shrilled just behind his head. Will the movement in process be suddenly energized and the cup thrown over his shoulder, or will the cup be dropped from his fingers? We should know enough about the rudiments of behavior to answer such questions without waiting for some drunken guest to conduct the experiment for us.

Certain recent experiments in conditioning applied a signal under circumstances which allowed the animal either to have the leg flexed or to have it extended at the time the signal was given. There is small wonder that the results were ambiguous, since response was bound to be ambiguous. Extension is impossible when the leg is already extended. Flexion can occur as an active response only when the leg is not already flexed. This reminds us also of the original admonition to note the facts of stimulus-response sequence. When a signal is alternately or at random presented during extension and flexion or during running and during cowering, but no record is kept of the response following the signal, we should not expect to find any definite generalization in our returns.

Horton and I had a very considerable amount of fact-trouble in making our observations of our cats. Though we made notes and in a number of cases a motion picture record, there was often doubt whether or not a sequence of movements of the cat in the box could be reported as substantially the same as a previous sequence. The statement just made that the major determiner of the animal's actions is the present state of action or rest is an interpretation rather than a fact. It is an interpretation to which we found ourselves compelled; but it is not an interpretation to which we could be sure other psychologists would be forced. A large part of the time we could tell at any moment what this particular cat would do next. Our ability to do so was based on having seen it execute the same routine in a previous trial or earlier in the current trial. Having started any former routine, we could predict its continuance. It is possible that this prediction should have been undertaken, to compare with prediction on any other basis. This seemed, however, a bit absurd, since we knew no other basis on which to base

predictions. The behavior of other cats allowed such prediction only in the most general terms, and did not apply to specific movement series.

The response-stimulus sequence to which I have been referring includes, of course, the familiar concept of "set." What I am urging is that the development of basic psychological theory demands an extension of our collection of facts in this class. We should extend radically our knowledge of sets and their consequences, and we should do that by observing what responses are elicitable from a given stance or set and what are not. Social psychologists are collecting facts about attitudes and their patterns, particularly those organized about words. This must eventually be reinforced by further theory of the elementary behaviors out of which attitudes are made. The watchful therapist is full aware of attitudes in his patient. The skilled mental tester learns to direct a child's behavior into attitudes that permit testing. We must eventually know more of the facts of these attitudes. What is the factual description of negativism or of resistance? To what extent are such attitudes general in their effects and to what extent specific? About what cues are they organized? What occurs when an attitude of resistance changes to one of acceptance? How do prevailing attitudes control attention and perception and learning? In other words, what effect does our own behavior have on our own behavior?

I am reminded here of an extremely interesting paper of Heider's on cognition recently published. In that she reports the results of some experiments which establish a certain hierarchy among concepts. Confronted with three sets of material in one of which the cognition is of an object, in another of a common form, and in the third, of a common number, the three are arrived at by subjects uniformly in that order—object, form, number. My own interest would be in the process through which cognition is attained. That process probably includes "trial and error" naming, and is not hopelessly un-get-at-able. My point is that cognition is not a mystery which involves a hidden thought process and a mental concept for which eventually a word is found, but is a case in which the subject uses his verbal repertoire in trial and error fashion until the word that "works" is hit upon. Proper search may discover that it is the careful noting of stimulus-response sequences that will furnish the factual basis for a theory of cognition. The cognition of objects may prove to be more ready than cognition of form or number because physical objects have a way of offering stable and recurrent, dependable, patterns of stimuli. Cognition of number may prove to be dependent on the initiation of counting which may be injected early in the process by an associative hint, or wait until trials

of object names prove futile. Concepts will turn out to be language in use, and use will turn out to follow the changes of learning by the same laws that will be worked out for action.

Up to this point a number of suggestions have been made for the direction of our search for psychological facts, for the ultimate purpose of understanding learning. One of these was that we have tended to neglect the behaving organism and to give undue attention to the external results of movement on the outer world. A second was that we would do well to recall that stimuli are the normal occasions for all response. A third was that we should note carefully the sequence of stimulus and response if we hope to get at the basic principles of associative learning. A fourth was that our interest in the role played by the organism in determining the response should be responsible for stern efforts to develop an objective, factual basis for our descriptions of the states of the organism that enter into the determination of behavior. A fifth suggestion was that closer attention to the response-stimulus sequence would be profitable in explaining motivation and the direction of learning.

My final concern is harder to name than these. I am in entire sympathy with the belief that quantitative treatment is to be aimed at in all scientific fact gathering. Number is the chief tool of science. But in our zeal to be scientific, I am convinced that we have been led into certain lines of experiment in the field of learning because these lines promised at least to yield numerical comparisons, curves. Because repeated trials can be given a series of ordinal numbers we have too readily fallen into the practice of treating the number of trials as a quantity, the more trials the better. We have been led to neglect what I am convinced is the central problem of learning, namely, what change occurs in behavior as the result of a single action.

In the laboratory we glory in experiments with fifty to fifteen hundred repetitions and their resulting curves. In nature these repetitions, as exactly duplicated as possible, simply do not occur. But learning does occur. The experimental results with a long series of repetitions have all the desirable characteristics of scientific fact. Their numerical analysis can be made by agreed methods. We must all recognize a mean and a standard deviation, or a difference and the standard deviation of a difference. And we are fairly agreed on the inferences that can be drawn from such analysis.

In the field of learning this very commendable effort to be scientific has led us toward studies of success, the trend of errors with repetition,



the reduction of time with practice. But it is a characteristic of a score of total errors (in a maze, for instance) to omit examination of the successive changes that constitute learning. Indefinite amounts of unobserved learning may enter into our final result. Before our criterion of success may be reached, learning, unlearning, relearning may have occurred over and over again.

We expect our friends to remember an engagement after one notice. We expect clinic patients to be different at each interview because of the last. We expect one quarrel to change attitudes. We expect one reading of a paper before this Association to leave some auditors with an impression. One spoiled egg may leave us for a time cautious. Once a rat has visited our grain sack we can plan on its return. Terror is called out once in the bird dog by the report of his master's gun, and the dog is now gun-shy.

But in the laboratory we assume that the response fixed in fifty trials was one fiftieth fixed in each trial.

Repetition has its place in learning, but repetition is effective only in those complicated instances in which what is learned is not a response to a stimulus, but a whole repertoire of responses to a large variety of stimuli. We have learned to achieve some result by means which vary according to the circumstances. Learning skills takes time and practice and furnishes beautiful learning curves and admirable data for statistical analysis. This is because they involve many and complicated learnings. It is here being suggested that the development of a scientific psychology requires that we investigate learning in its simplest forms. What happens as the result of one pairing of a stimulus pattern with a response that alters the previous effect of that pattern?

No group of psychologists has done more toward investigating this phenomenon in its elemental form than the Yale group under the inspiration of Clark Hull. With that work I have only one quarrel. This is that they have not examined adequately the R-S sequences which I have mentioned. Hull's theory, which has dominated the collection of facts in the Yale laboratory, is the theory of reinforcement, not a straight associationism. It assumes that an association is formed, or is not formed by virtue of a subsequent reinforcement or reward which somehow works upon traces of the S-R event and confirms or destroys the associative connection.

This theory is in line with the great tradition of the psychology of learning. Thorndike in his *Animal Intelligence*, C. Lloyd Morgan in his book of the same title, and Hobhouse in his *Mind in Evolution* all

speak in terms of a confirming reaction, which determines whether or not the association will be made.

No one questions the effectiveness of reward and punishment, or the effect of after-effects of a reaction on learning. But this statement of learning theory has led to an entire neglect of the observation of R-S. The confirmation or reward or punishment is supposed to have its effect by virtue of simply being confirmation or reward or punishment, not by virtue of the effect which it has on the stimulus situation and therefore on subsequent behavior and the opportunities for further learning. There is excellent reason for believing that both reward and punishment are effective by virtue of what they make the animal do, not simply by virtue of their own nature. Adherence to the theories of confirmation or reinforcement has led to quantitative results, it is true. It is highly probable that close examination of the action caused by punishment and the action caused by reward will discover that the learning which takes place can be adequately described in terms of the new associations set up by the new action. Reward, as Thorndike has remarked, tends to leave the animal doing the same thing in the same situation,—eating while food is present. Punishment induces the animal to do something different in the same situation. A theory of associative learning in its straight form without appeal to after-effects would lead us to predict in these instances what happens. The animal does not unlearn its tendency to do what it previously did if rewarded because nothing has happened to establish rival responses to the situation. It does not learn not to eat when the food is finally presented although it does eventually desist, because either the food or the inner hunger is now absent and cannot be re-conditioned in their absence.

Culler's laboratory, like Hull's, has led in the investigation of relevant facts on learning in its elemental form. Some of that work I should like to see repeated with closer adherence to the S-R prescription. Stimuli are applied without observing what the animal's actual next behavior is. In a number of instances two rival responses, flexion and extension, take place following the signal. That the result of this mixed practice turns out to be not a straight exemplification of association is not to be wondered at.

In general the experimenters who work with what has come to be called instrumental conditioning also fail to observe the maxim to observe the response following the stimulus. It would seem obvious that an investigation of association would require in the first place that the stimulus and the following response be at least made a matter

of record, but experiments in instrumental conditioning seldom record what the animal was doing when the signal was given. The returns therefore throw no light on association, but only on the effects of reward on subsequent response to the signal. In these experiments not only does the S-R fail to be observed, but also the sequence, R-S'. None of the experimenters is interested in the immediate behavioral consequences of reward, but only in the remote effects of reward upon a previous stimulus.

May I here recall the initial theme of this paper. It has been concerned with the future development of psychology as a science and particularly with the possible effects of a sudden increase in the numbers of psychologists and a sudden enormous extension of the application of psychology to practical affairs. None of us doubts that human living will be improved by that extension. Most of us would accept that improvement as the final goal and justification of all human science. But we must remember that the sciences have developed through an objective detachment from immediate profit, and that, in the overwhelming majority of instances, steps forward in scientific theory have been independent of practical application.

The hope that is here being expressed is that the new psychologists will in general not allow themselves to become mere technicians using psychological methods and techniques for the accomplishment of practical ends, that in the training of the new generation of psychologists we take care to cultivate an interest in theory as well as practice. We are entering a period of increased usefulness. It is to be hoped that it will not be a period in which theory stands still. Our factual information is bound to increase at a greatly accelerated rate. For that increase to result in the advance of psychology as a science two things are necessary. One is that theory be continuously produced and continuously amended and continuously used to guide the collection of fact. The other is that we remember to conform to the rules that have been responsible for the remarkable achievement of the scientific tradition, the use of objective evidence which means a basis in facts open to the observation of all who are interested and described in public terms that must be accepted by other scientists. These requirements may bear heavily on many current movements in psychology, in which recognition of events is claimed to be an art not communicable by ordinary means, open only to the inner members of a cult and closed to outsiders. Facts may accumulate without theory; but they will prove to be unstable and of little profit in the end. Theories may flourish if their basis lies not in scientific fact

but in opinions and interpretations acceptable only to the members of a limited faction; but they will be bad theories. Schools flourish only when theories are not carried back to public facts. Unless psychologists maintain an interest in general theory the fields of psychology will increasingly become independent collections of undigested information.



## SHOCK THERAPY: PSYCHOLOGIC THEORY AND RESEARCH

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### INTRODUCTION

For more than a decade contemporary psychiatry has been employing extensively as a treatment for psychiatric illness the procedure of artificially inducing comatose and convulsive states in mental patients.

It is not at all difficult to find historical antecedents in the care and management of the psychiatric patient which superficially, at least, appear to be like the current methods of shock therapy in that a sudden and sometimes quite aggressive change is effected in the subject's environment leading to a precipitous alteration in the patient's physical and psychophysiological state. The content of these methods varied with the change in cultural thought and are, indeed, as adequate symptomatic reflections of the history of psychologic thinking as, for example, are the historical changes in the interpretation of dreams or the evolutionary variations in the concept of the soul.

Occipital branding was perhaps one of the earliest of these methods, and the variations of the well-known water cures wherein the "frantic person was placed with his back to the water without being permitted to know what was going to be done" and who was then "knocked backwards into the water by a violent blow on the chest and tumbled about in a most unmerciful manner until fatigue had subdued the rage (159)" have a long history of therapeutic existence. Even in the time of Herman Boerhaave (1668-1738) "ducking" was a common psychiatric procedure as it was, too, in the remedial repertoire of Benjamin Rush, the father of American psychiatry. Additionally, Boerhaave recommended a special twirling stool on which by spinning a patient until he became unconscious his brain could be "rearranged" and the patient made normal (159).

The Roman, Cornelius Celsus (14 A.D.), in his *De re medica* wrote that a certain amount of physical coercion and a measure of gentle cruelty and the more refined tortures of inducing fear might bring the mental patient to his senses. Phillipe Pinel, the famous psychiatric emancipator, also believed in fright as an effective psychiatric remedy, and Johann Reil (1759-1813), while prescribing throwing patients into water and subjecting them to the firing of cannon, also was a protagonist of other forms of non-injurious torture (159).

Certain followers of Asclepiades, like Themison, approved of large doses of alcohol, antedating by two thousand years the therapeutic efforts of Kantorovitch, Constantinovitch (75), Berrington (12) and others who have relatively recently attempted to modify schizophrenic stupor by brandy taken orally or by alcohol given intravenously. These students of Asclepiades were also convinced of the merits of whipping the mental patient as were also the Anglo-Saxon priests described in *Bald's Leechbook* who believed that if one took the

skin of a porpoise and worked it into a whip and then would "swing the man therewith, soon he will be well. Amen."

Blood-letting, perhaps reaching its height as applied to psychiatric treatment under the Parisian, Édouard François Marie Bosquillon (1744-1816), was also widely practised over a considerable period of time and must undoubtedly have produced in some patients surgical shock if nothing else.

Following hard upon the heels of the creation of the Leyden jar, the first electrical condenser, by Pieter van Musschenbroek in 1746, Richard Lovett 10 years later claimed success in treating mental diseases with electricity. Arndt (7) in 1870 reported good results in depressions after electrical shocking, and other investigators, notably Huff (67), Haynes (67), and Allbutt (4), favorably inclined toward the psychiatric therapeutic use of electric current.

Cyanide, which among other effects depresses brain metabolism, had been used by Loevenhart (65) as recently as 20 years ago to obtain remissions in mental patients. Much earlier, hellebore had been extensively used, leading to the term "helleborism" as meaning a form of psychiatric treatment.

Hippocrates had in his times observed that organic disease supervening upon mental illness sometimes caused an abatement of psychotic symptoms, and remissions in psychiatric patients after surgical anesthesia, aborted suicide, fractures, and intercurrent disease are known to every psychiatrist who has had any extensive institutional experience.

The enumeration of these historical forerunners of contemporary shock treatment, however, does not assume any necessary continuity or similarity of motivation in the use of shock therapy, unless one may speculate upon the possible common presence of the less consciously defined motivation of aggression against the patient evoked in the physician by the inadequately met challenge of the etiological and therapeutic demands of the patient's illness, and one may perhaps also tentatively consider the incidental motivational importance of the residual in the therapist's personality of child-attitudes of the expectancy of the sudden, miraculous, magic resolution of puzzling barriers.

Shock therapy, whatever the ultimate evaluation of its therapeutic efficacy or desirability may be, provides extraordinary research opportunity under fairly controlled conditions for the investigator interested in either the psychologic or the neurophysiologic descriptions and interpretations of the multitude of happenings associated both with the active process of treatment and with the immediate and remote results of these therapeutic shock procedures.

For the psychologist, the controllable creation of convulsions in the human subject makes possible, for example, a precisely defined investigation of personality reintegration after convulsive dissolution. Many of the characteristics, both theoretical and actual, of psychological ontogenesis may be studied in this reintegration of the personality. Disturbances in perceptual organisation and the relation of such disturbances to general cognitive functioning, the description of the de-

terminants of perception, the relation of memory-content to problem-solving, learning, and logical thinking, the relation between emotion and memory and the relations between emotion and various personality dysfunctions, and the investigation of general problems of learning and habit-structure are but a few of the subjects which may be studied, from perhaps new aspects, by the observation of post-convulsive behavior.

The psychopathologist will be interested in what he may recognize, as did Sakel, as "activated symptoms" of a psychosis, or he may see symptoms suggestive of failure of inhibition and control and of the release of ontogenetically earlier modalities of behavior. So, too, the relationship of the changes in the person which are produced by convulsions to the characteristics of the pre-treatment psychotic personality merit closer scrutiny than has as yet been reported. The field of prognostic evaluation and psychological aid in the selection of candidates for successful therapeutic issue has been more widely studied. Systematic psychologic comparisons among convulsive and comatose reactions produced by the various precipitating agents and the examination of the similarity and difference of the after-effects of these induced convulsions both with reference to the varieties of artificial induction and to the immediate and remote sequelae of endogenous convulsions have not yet been adequately accomplished. Some of such comparative evaluation, however, is now much less important because of the empirical selection of electrically induced convulsions as the currently preferred treatment in most cases, with insulin shock now considered as possibly primarily valuable in certain kinds of schizophrenia. Psychologic assessment of what has been the nature of the personality dynamics in the cases of remission and recovery needs to be increasingly critical as a basis for the rationalisation of the therapy. The psychologic as well as the neurophysiologic interpretations of the therapeutic efficacy and mechanism of shock therapy must be substantiated progressively by objective research.

Neurophysiology is interested in the happenings which afford an opportunity to adduce facts confirming or depreciating theories of nervous system function and of the neurophysiologic correlates of behavior. Artificially induced convulsive reactions exhibit various degrees of neurophysiologic dysfunction and these shock-induced syndromes appear to be reversible. Critical attention directed to these conditions provides neurophysiology with a pathological analysis of the nervous system as well as affords an opportunity to attend more closely to the neurophysiology of mental disease.

Since the literature directly and indirectly relating to shock ther-

apy has become so extensive, the formulation of future psychologic research and theory may be aided by a summary and critical review of pertinent psychologic description and interpretation evolving out of the widespread application in psychiatry of these recent therapeutic techniques.

### SHOCK THERAPY

#### *Induction Methods and Characteristic Reactions*

**Insulin.** In 1928 Manfred Sakel (124) began to treat abnormal mental conditions by insulin-produced hypoglycemia after his experience in treating drug addicts for abstinence symptoms with insulin in order to depress the "dominatingly increased" activity of the sympathetic nervous system which he considered was associated with the withdrawal effects. In 1933 in the University of Vienna Psychiatric Clinic he began to treat schizophrenics by induced hypoglycemic reactions. However, it had already been discovered in Oslo (83) quite by accident that a mental patient could be "cured" by the induction of hypoglycemia, and Day and River had independently made the same discovery here in America. An Hungarian doctor also claimed to be using insulin in the treatment of mental conditions a few years before Sakel initiated his treatment.

Sakel's original procedure of treatment has undergone many modifications, but, in general, sufficient insulin is given to induce first quietness, then drowsiness, sleep, and finally coma. A comatose reaction has ensued in an individual with as little as five units of insulin and has not followed in another patient even after 400 units of insulin. The variability of individual behavior in the hypoglycemic reactions is considerable. Some patients evidence great restlessness and even excitement before going into coma, while others may attain the comatose state without much expressive activity at all. From the physiological aspect, sweating, increase in pulse rate, and muscular movements may be seen in the pre-coma. Isolated twitching and jerks in the body musculature may then merge into a generalised convulsion, although usually the coma is interrupted before a convulsion eventuates.

Sakel writes of the cerebral functions gradually disappearing, level by level, as the hypoglycemia intensifies and then returning as the patient awakens within the quarter hour after termination of the hypoglycemic reaction by the administration of glucose. Frostig (42) has further detailed the neurologic description during five hours of the hypoglycemic process. In the recovery period confusion, psychomotor excitement, dysphasia and other disturbances are seen. Most patients are amnesic for the period of hypoglycemia and where the coma has been



prolonged or where a convulsion has occurred retrograde amnesia is observed. In this latter respect it is interesting to know that Sakel in his early studies attached a certain therapeutic importance to epileptic and epileptoid motor reactions and stressed the necessity of long treatment not only for the modification of secondary symptoms but also for the control of the symptoms of the "activated psychosis" which Sakel thought to occur at a stage in the treatment when the patient showed no signs of his psychosis except during the hypoglycemic state. However, as Kalinowsky (70) particularly points out, the acute psychotic picture which Sakel called the activation of the psychosis is undoubtedly a transient organic reaction to the treatment and very probably would occur in a non-psychotic subject as it does, indeed, in psychoneurotic subjects.

Early in the development of insulin shock therapy the Viennese school stressed the importance of a period of euphoria, accessibility, and inhibitory release in the early hypoglycemic period, although the occurrence of such behavior is by no means a constant feature of insulin shock induction. Psychotherapy was used during the existence of this condition as well as in the post-coma phase of recovery. Such transitory insulin-induced accessibility may be compared to current narcoanalytic technics using soluble barbiturates. Bleckwenn (15) had in 1930 used sodium amytal in the treatment of the psychoses. In connection with the mention of the narcoanalytic technic it is interesting to observe that Thomas (143) and others have reported that mute and inaccessible psychotic patients who respond to intravenous sodium amytal will also respond to shock therapy and Thomas has proposed the use of sodium amytal as a prognostic aid in predicting the outcome of convulsive therapy.

There is no space in this paper to discuss the possible neurophysiological mechanisms of the psychosis-modifying effect of insulin treatment. Sakel (125) thought that adrenalin hyperactivity produced a lowering of neural thresholds in the cerebrum, thus reviving the phylogenetically oldest and infantile constellations and pathways among the neurones. Insulin neutralises the effects of adrenalin and was thus considered to reverse the whole process, the resulting hypoglycemia producing a vagotonia. Gellhorn (44) believes that insulin leads to an excitation of the sympathetic-adrenal apparatus through hypoglycemia of the brain, and Heilbrunn's (58) experiments are in favor of the hypothesis that insulin creates a sympatheticonia, although Hadorn (56) has indicated that such effects may depend upon the dosage and he has demonstrated that small doses of insulin stimulate the vagus while large doses cause a primary secretion of adrenalin. Pfister (58) thought

with Sakel that hypoglycemia damps the sympathetic nervous system.

However, as Riess and Berman (117) have indicated, there is no evidence of adrenalin overproductivity in schizophrenia or in other psychotic states. Insulin, in fact, increases adrenalin in the hypoglycemic phase. Parker (108), too, has shown that the autonomic functions during the hypoglycemic states are marked by oscillation between sympathetic and parasympathetic tonus, due to the increased activity of one system rather than a diminished tone of the other. Moreover, this investigator found that recovery occurred both in cases where chiefly sympathetic and where primarily parasympathetic excitation was evident. Fortuyn (37) also observed both sympathetic and parasympathetic activity in the hypoglycemic state.

Other explanations have stressed, like that of Georgi (45), the decrease of permeability of neuronal membranes in the psychoses facilitating the retention of toxic products, a condition which is then altered by the insulin effects. Assumptions have been made, like that of Demole (28), which are based upon a belief that insulin tends to correct disturbances of neuronal oxidation which results in an accumulation of suboxidised products of metabolism which produce schizophrenic symptoms as a form of cerebral toxemia. Unfortunately, these descriptions point to an insulin effect upon an assumed but unproven physiopathology of the psychoses.

*Metrazol.* This is a synthetic compound clinically related to camphor. In this substance the six carbon ketone ring of camphor is condensed to form pentamethylenetetrazol (53). Although Paracelsus had used camphor as early as the 16th century to produce convulsions and although Oliver in 1781 treated a manic psychosis by camphor-induced convulsions, cardiazol, as the synthetic compound is called in England and on the continent, was synthesized and first investigated pharmacologically by Schmidt, Hildebrandt, and Krehl (127), and it was soon being used as a stimulant in circulatory and respiratory collapse. In 1926 Blume (16) found it possible with large doses to produce convulsions in animals.

Meanwhile, a few years before the use of psychiatric convulsive therapy was begun, Nyiro and Jablonsky had observed in 1929 that cases of epilepsy in which schizophrenic features were prominent recovered rapidly from the schizophrenia when fits were frequent, and in 1930 Müller (97) described two cases in which schizophrenic illness recovered quickly after the appearance of spontaneous epileptic fits. Glaus (49) had also reported a year later that the combination of schizophrenia and epilepsy was rare, although such an observation was certainly contra-

dictory to textbook descriptions of the well-known continental psychiatrists of the time, like Kraepelin and Bleuler, who found nothing extraordinary about the diagnosis of schizophrenia with epilepsy. However, in contrast, it is interesting to note that Steiner and Strauss, writing in Bumke's well-known *Handbuch der Geisteskrankheiten*, would question the correctness of the diagnosis of schizophrenia in any case with convulsive attacks.

Glaus investigated 6,000 schizophrenes and found that only eight had had fits at any time in their lives and, more interestingly, he discovered that four of these eight patients recovered shortly after the fits appeared. Other scattered reports in the literature also indicated a possible antagonism between schizophrenic catatonia, at least, and epilepsy.

In 1934 von Meduna, the director of a mental hospital in Budapest, who had been experimenting with camphor-induced convulsions in animals, began to produce convulsions in chronic schizophrenics by the intramuscular injection of 25% camphor in olive oil, being motivated largely by the consideration of the possible salutary effect of epileptic seizures on the schizophrenic illness. In addition to what he considered the incompatible occurrence of epilepsy and schizophrenia in the same individual, Meduna also sought to establish that epileptics belong chiefly to Kretschmer's athletic type and that the neuroglia in epileptics is hyperplastic in contrast to the supposed neuroglial aplasia of the schizophrenic. The carbohydrate metabolism was also evidenced to be slowed in schizophrenia and accelerated in epilepsy.

Against such evidence of the antagonism of schizophrenia and epilepsy there have been several observations published in recent years. The paper of Yde, Lohse, and Faurbye (158), for example, recounted the data of 715 cases of schizophrenia 20 of whom had had convulsive attacks, and epilepsy was established in five of these cases, which is about twice the incidence expected by chance. Hoch (66) has also indicated a positive tendency for epilepsy and schizophrenia to be associated. Moreover, the standard classification of mental disease, in this country at least, may lead to serious statistical collection errors in that schizophrenic symptoms occurring with epilepsy are very likely to bear the diagnostic label "Psychosis due to convulsive disorder (epilepsy)" and so completely escape the attention of the searcher who looks only for schizophrenia with an accompanying epilepsy. As this standard nomenclature indicates, the organic bias of contemporary institutional psychiatry will probably put the major diagnostic emphasis upon the epilepsy.

Most metrazol convulsions are now induced by the intravenous injection of the drug so that the usual interval between the commencement of the injection and the initial signs of convulsion is about ten seconds. The reaction may be either subconvulsive or generalised and is usually evidenced by sudden pallor, a short cough, muscular twitching, and a terrified expression, and sometimes by a cry or scream. Unconsciousness after the fit lasts for variable intervals and the post-convulsive recovery is characterised by the same sequence of events as occurs after any generalised convulsion.

Intramuscular injections of triazol produce a slower prodromal convulsive picture than intravenous metrazol and considerable expressive activity may be seen in the patient before the convulsion is precipitated.

Multiple convulsions may be produced by large doses of cardiazol.

Denyssen and Watterson (29) believe that the convulsion following cardiazol injections is due to sudden vasoconstriction, but Georgi (46) thinks that the pathogenic factor in the initiation of convulsion is not to be found in a primary vasospasm but in the ionic change at the cell membranes, and he has suggestively demonstrated that the ionic changes occur only when the cardiazol injection is sufficient to produce a generalised convulsion. This is possibly significant in that it is generally accepted that subconvulsive reactions are therapeutically much less effective than are total convulsive reactions, although the contention of Polatin, Spotnitz, and Wiesel (116) that in the insulin procedure hypoglycemia alone without coma or convulsion is adequate to bring about remissions must be kept in mind. This observation is also substantiated by Krasnouchkin and Hanlarian (82) who obtained very favorable therapeutic results both with and without provoking reactions of coma. Hill (64), too, has claimed success by treating schizophrenia and other mental diseases with small doses of insulin (5-10 units) and histamine. Hill proceeds on the assumption that there exists in psychiatric disease a pathologic barrier in the endothelial walls of the capillaries against the normal interchange of plasma. Histamine, particularly, is considered to affect this pathologic barrier so that capillary walls are made more permeable to the osmotic cell-nutrient interchange.

*Ammonium Chloride.* Bertolani (118) in 1938 began producing epileptiform seizures by the intravenous injection of a solution of ammonium chloride. This method has been very little used in the United States, but it enjoyed a temporary popularity in India in the hands of Rizvi (118) and on the continent and in England it had a number of protagonists, including Mazza (118), Dax (26), Martinez and De la



Vega (89), and others. The reaction usually consists of a pre-convulsive period lasting 10-20 seconds in which the face becomes flushed, and in which hyperpnea, pupillary dilatation and loss of reaction to light, and absence of corneal reflex are seen. The convulsive phase proper is usually mild but may be typically epileptic, although ordinarily only muscular twitching and extension and flexion of the legs and arms are observed. No cyanosis, incontinence, or biting of tongue is commonly present. Afterward the patient may be disoriented. It is assumed by Rizvi that the convulsive reaction is produced not by alteration of the blood pH but by the irritant effect of the ammonium chloride on the blood vessels, causing vasoconstriction.

*Coramine.* 12 to 15 intravenous injections of 10-25 cc. of coramine (Nikethamide) solution, a drug which like metrazol is a central nervous system stimulant, have been used as a course of treatment for psychiatric syndromes. Following injection, an initial apnea results followed by hyperpnea, accelerated heart rate, increased blood pressure, profuse sweating, tremors, and nystagmoid ocular movements. Consciousness is retained, but confusion, disturbance of attention, and general "clouding" of mental function may last for about 10 minutes. Anxiety and fear expressions are seen. Skorodin (131) claims 19% remission obtained in selected cases.

*Electric Shock.* The currently most widely used shock procedure originated from the use of electricity in producing convulsions in dogs. Leduc had in 1900 produced electrical sleep in animals, and Robinovitch (119), working in Leduc's laboratory, found that her electrical stimulation of the dog's brain sometimes produced epileptiform convulsions, an occurrence which five years later was confirmed by Weiss (152). In 1929 Viale (145) was inducing epileptic seizures in dogs by placing electrodes in the mouth and rectum and passing an electrical current. Cerletti (21), who had also had extensive experience in the production of electrically induced convulsions in animals, inaugurated in 1938 with his collaborator, Bini, the electroshock treatment of the human psychotic subject.

Four hundred to 500 milliamperes of alternating current are usually passed for about .2 second through electrodes placed on the fronto-temporal cranium. Subconvulsive reactions or generalised convulsions ensue concomitantly with the passage of the current. Wilcox (153) conveniently divides the electroshock convulsion syndrome into an initial tonic phase followed by a clonic phase, an atonic phase, a stuporous phase, and a post-convulsive mental state.

The voltage of alternating current necessary to produce an effective

stimulus is usually more than 100 volts. Cortically initiated muscular movements may be evoked by less than 10 milliamperes when the stimulation is applied directly to the brain, but in electroshock procedures the intervening skull and tissues as well as the reduction of current density because of diffusion of current lines of flow make a relatively high voltage necessary. Also as Hemphill and Grey Walter (61) point out, the alternating current stimulus is repetitive. Since electrophysiologists believe that the physiologically important stimuli arise at the negative pole of the applied current, an adequate electrical tension applied for .2 second in 60 cycles of alternating current will result in 12 effective stimulations of each cerebral hemisphere. The duration of current application, therefore, is the moderator of the number of times the brain is electrically stimulated. The smaller the number of current volleys, the greater is the current strength necessary to induce convulsion. It has also been shown by a number of workers that Ohm's law does not apply to the passage of current through tissues. With the flow of current the initial resistance is modified by the factor of reactance and possibly by capacitance and other determinants so that the measurement of resistance is of little value in calculating current effect.

Friedman and Wilcox (41) have used unidirectional wave forms of current, including half-sine waves, galvanic current pulses, combinations of these two forms, and repetitive condenser discharges. It has been stated that some alterations in the wave form of the current produce the convulsive effect with less amperage and lessen the post-convulsive disorientation and dysmnesia.

Berkwitz (10, 11) introduced an electrical shock method in which the stimulation is effected by about 30 shocks of faradic current from an induction coil, one half second in duration, with current strength under the convulsive level. Two to 45 such treatments are given in which fear and some pain are characteristic reactions. He reports in one study (11) that 35% of chronic patients so treated showed improvement, and one complete remission was obtained.

*Electronarcosis.* Reminiscent of Leduc's electrically induced sleep, the introduction of an electronic apparatus delivering 160-250 milliamperes of 60 cycle alternating current continuously in the human subject for an initial 30 seconds through bitemporally placed electrodes has been a recent modification of electroshock therapy. The first 30 seconds of stimulation result in a tonic spasm with a few seconds of cardiac arrest and a 30-45 second respiratory suspension. The current is then dropped to 60-70 milliamperes, whereupon a few mild clonic

movements may be seen, and then after 60-75 seconds from the initial application the current is increased by a five milliamperere increment every 15 seconds to a maximum of 125 milliamperes at five minutes. The major differences from electroshock reactions are seen in a prolongation of stimulation of the autonomic nervous system, in prolonged flexor tone, and in the presence of forced grasping. These kinds of reaction are seen, however, in deep insulin coma, and Thompson (144) and his co-workers believe that electronarcosis is similar in therapeutic efficacy to insulin treatment and that it is superior to electroshock in the management of schizophrenia.

*Other Methods.* The employment of picrotoxin, anaphylactic shock, nitrogen anoxia, carbon dioxide stimulation, refrigeration, sterile meningitis, and a few other methods which have been used experimentally in an effort to modify human psychoses need not be discussed since they have not been used extensively enough to have provided any psychological data relevant to the interests of this review. Psychic "shock" is also being advocated by at least one current writer (35).

*Prolonged Narcosis.* Continuous narcosis of several days' duration is used most frequently in contemporary psychiatry in the handling of manic excitement and in the treatment of certain battle reactions. We must go back at least as far as MacLeod (62) for historical priority in the use of *Dauerschlaf* in the treatment of mental disease, although Locock in 1857 had advocated the use of bromide in the treatment of epilepsy and in neuroses, particularly hysteria. MacLeod, working in Shanghai in 1897, began the treatment of acute mania with bromide-induced narcosis of several days' duration. Previously, of course, the ancients had made conflicting observations upon the value of sustained sleep in mental disease, and in the middle nineteenth century Andral and van Swieten among others had seen maniacal conditions cured by accidentally large doses of opium. In 1901 Wolff (62) treated confusional states with trional, and Epifanio (62) in 1915 was using luminal. Kläse was encouraged by the results of somnifaine narcosis which he had induced in 26 schizophrenics and which he reported in 1922.

Prolonged narcosis is not, strictly speaking, a "shock" therapy, but a brief consideration is necessary because earlier it was used for all kinds of psychiatric disorder and reports, for example, like that of Palmer (106) who in a mixed series of cases treated with somnifaine obtained 33% recoveries are psychologically significant in evaluating the specificity of the nature of the shock-induced remissions. Meerloo (93) also obtained one-third recoveries in 500 well-mixed cases of psychiatric

disorder following prolonged barbiturate narcosis. Hennelly (62) breaks down his remissions to 42% in mania, 32% in melancholia (manic-depressive-depressed), and 44% in involutional melancholia.

*Treatment Combinations.* Early in the use of insulin, the hypoglycemic coma was interrupted by metrazol convulsions in order to obtain the therapeutic effect of the epileptiform seizures. Electric shock has also been used in conjunction with insulin. Moreover, failures with one procedure have been subjected to other kinds of shock therapy. There are psychiatric patients even who have initially undergone insulin shock, then a series of electroshock treatments, and who have then finally been subjected to frontal leucotomy. One of Freeman's and Watts' patients, quite interestingly, passed through a psychoanalysis, an insulin shock treatment, and two frontal leucotomies, and she is currently engaged in working on her doctorate thesis (39).

Another  
shock,  
no  
doubt

Adequate statistics and careful definition of the characteristics of pre- and post-treatment personality in these cases where more than one shock procedure has been used would be most helpful in the evaluation of the effects of these different shock methods. However, the literature is rather disappointing in this respect. Patients who have not responded to one kind of shock procedure may occasionally benefit by the application of another convulsion- or coma-inducing method. However, where such a change in the treatment technic is made with favorable results, this successful issue may in many cases very probably be due to the total length of the combined treatment and not to the change in procedures, since some patients may not show adequate modification of the psychotic symptoms until after they have experienced 20 or more convulsions. There is some indication that insulin is more effective than other methods in the treatment of schizophrenia, and hence it may very well occur that some of such cases may show a disappointing reaction to metrazol or electricity and yet improve as a result of the hypoglycemic regime.

More adequate research substantiation is needed before the superiority of any one shock procedure is established for the treatment of any specific psychiatric syndrome. More and more it appears that the intensity of treatment, of whatever kind, is the important consideration, and it may very well be, as Kalinowsky (70) suggests, that even manic excitement may be controlled by shock therapy provided the confusional form of treatment described by Löwenbach (86) is used, although Löwenbach, himself, believes that convulsive treatment is of little value in mania.



Illustrative of encouraging results obtained by a change of method is the report of Weinberg and Goldstein (151) who observed that 32% of 100 patients treated with insulin following failure to respond to metrazol showed good improvement.

*Combination with Other Drugs.* Various substances, such as curare, beta-erythroidin hydrochloride, or quinine methochloride may be combined with the convulsive agent in order to lessen the motor severity of the convulsion. Shock treatment has also been given with the patient under general and spinal anesthesia. Sodium amytal may be used to aid in the control of the resistive and apprehensive patient and in order to forestall post-convulsive excitement. Scopolamine has been similarly used. Cocaine has been employed to induce a pleasant post-convulsive emotional state. Magnesium sulfate has been combined with convulsion-evoking agents, and strychnine has been used to increase the sensitivity of the nervous system to metrazol.

*Combination with Psychotherapy.* Considerable controversy exists among psychiatrists as to whether shock treatment should be combined with active psychotherapy. In the case of any formal analytic process demanding continuity of progress and of the experience of therapeutic "movement," this problem is solved rather incontrovertibly in most cases by the inability of the patient to remember either the analyst or the content of the analytic sessions from one treatment day to the next. However, in many places where restrictions on time and personnel are not too drastic pragmatic psychotherapy is usually attempted. Even if the patient has amnesia for the period during which psychotherapeutic relations could be established, such interpersonal experience may quite conceivably be of considerable therapeutic importance even if it is not immediately reflected in prognosis. We must be aware, however, that Myerson (98), for example, allowed some of his ambulatory patients to be treated by electroshock with minimal personal contact, and he reported as favorable results with this group as with patients additionally aided by psychotherapy. Nevertheless, Orenstein and Schilder (102) pointed out some time ago that there is a difference in the kind of insight socially recovered patients may have, depending upon whether they have become aware of the psychodynamics of the self through the process of psychotherapy or whether they simply know, following successful shock therapy alone, that they were once ill and are now, somehow, well again.

The failure of later investigators using the insulin technic to achieve the good initial results reported by Sakel may be due in part to differ-

ences in the intensity and quality of psychotherapy employed by the different clinics. Sakel himself was willing to attribute 20-30% of the total insulin-induced remission percentage to the concomitant use of psychotherapy.

Convulsive therapy, because of the post-convulsive accessibility and helplessness of the patient, may provide the first opportunities for the establishment of psychotherapeutic relationships with a mute, inaccessible, or negativistic subject. Moriarity (65) believes also that the combination of convulsive therapy with psychotherapy yields better results than psychotherapy alone in treating the neuroses and the borderline psychoses. This observation, however, must be tempered by the consideration of the general agreement of opinion that shock therapy alone is of little or no value in the treatment of the neuroses, with the possible exception of the reactive and anxiety depressions and some of the more blatant hysterias.

*Duration of Treatment.* Insulin coma is usually induced six days weekly for at least one month so that as many as 30 to 40 single treatments may be administered and in some cases, many more. Metrazol and electricity were used generally to induce convulsions about two or three times weekly to a total number of 10 to 15. The emphasis soon came to be placed upon an adequate number of convulsions (at least 20) and upon a more frequent occurrence so that currently several electroshock convulsions may be administered even in the same day. Löwenbach (86), for instance, has stressed the necessity of achieving long-lasting disorientation in psychotic subjects by frequent convulsions as one requisite for successful therapeutic effect. Freeman (39), also, considers frequent convulsions and the resulting enduring disorientation desirable. However, the decision about the spacing of convulsions depends upon what the effect upon the patient of shock treatment is thought to be as well as upon the personality reaction being treated.

#### OBSERVATIONS OF IMMEDIATE TREATMENT REACTIONS

*General Clinical Description.* The actual behavior of patients reacting to convulsion-precipitating agents has been described with considerable agreement by many observers. Löwenbach and Stainbrook (87) have stated that a generalised convulsion results immediately in a state in which it is impossible to demonstrate operationally any behavior subsumed under the psychological conception of personality. "The individual does not react to any kind of stimuli and the activity of the electroencephalogram has almost ceased." The convulsion is then succeeded by the gradual return and reintegration of personality functions.

According to these writers, "it seems as if the subconvulsive reaction does not differ from the convulsive except in degree, but in these minor reactions . . . reintegration may begin at any level and the recovery may be much more quickly and indeed sometimes imperceptibly achieved." Cohen (23), studying the return of cognitive function following metrazol convulsion believes that the continuum of recovery can be rather adequately defined in terms of the initial and immediate post-convulsive anesthesia, aprosexia, agnosia, apraxia, and amnesia. After four minutes usually, tactile, visual, and other sensations begin to show evidence of function, and there then follows in roughly chronological sequence but with much overlapping, of course, and with progressively decreasing exhibition of dysfunction, the recovery of attention, gnosis, praxis, and memory. Stainbrook (137) writes that the cognitive behavior observed clinically following electrically induced convulsions may be considered conveniently as consisting of immediate transitory effects most economically conceived as reflections of severe neurological dysfunction and of a more remote and relatively enduring symptomatology of disorientation and dysmnnesia.

Ismael's (69) observations on patients during hypoglycemic induction recorded anomalies in sensory perception, including hypnagogic visions and hallucinatory images which were primarily visual but which occurred in all sensory spheres. He also saw what he inferred to be anxiety, terror, euphoria, rage, and panic. Misidentification of persons existed as well as disordered gesticulation and dyslalia. Evidence of exteriorized sexuality in the forms of homosexuality, sadism, and exhibitionism was also observed. Like von Angyal (5), Benedek (9), Palisa (105), Plattner (114), and others, Ismael was also able to demonstrate disturbances of the body-image. These latter investigators found that the hypoglycemic state produced changes in the perception of space and of color qualities and disturbances in gestalt comprehension and in concept formation. Pisk (113) described the appearance of the "Zeitraffer" phenomenon in insulin coma and reported a patient who experienced acceleration in the flow of time. Orenstein and Schilder (102) also report alterations of time perception occurring during insulin reaction. Benedek (9), quite interestingly, detailed disturbances in the perception of movement which were similar to those observed by Pötl and Goldstein and Gelb in occipital lobe organic lesions. Benedek also reports that micropsia, megalopsia, and primitive optic hallucinations appeared and that colors appeared less impressive to patients during shock and looked more saturated after shock was over.

Silbermann (130), from his studies on subjects treated with insulin

and triazol, describes two groups of symptoms, one group being classified as regressive and occurring in the interval between the injection and the onset of the coma or convulsion and one group being seen after "shock" and described as restitutive. Among the regressive symptoms he would place feelings of giddiness and ill-defined feelings of apprehension, sensations of hot flushes, excitability, disturbances in the evaluation of shape, distance and size, abnormal sensations of color, distortions of auditory impressions, feelings of unreality, guilt and fear of punishment, confusion, loneliness, feelings of world-destruction, and fear of death. The immediate post-shock, or restitutive, group of symptoms includes, characteristically, feelings of fear, confusion, and unreality as well as motor and sensory aphasia, euphoria, and a sense of being like a helpless child. Good (51) has also described the pre-convulsive aura following metrazol injection as evidencing apprehension, perplexity, strangeness, and choking sensations and as culminating in intense fear and terror. Friedman (40) in the same pre-convulsive reactions saw "rapidly mounting anxiety which in certain cases rose to a vivid deliriform panic-state with concomitant characteristic vasomotor and psychomotor overactivity." Collins (24), too, stresses the emotional responses of fear and apprehension in the aura of metrazol convulsions and in the hypoglycemic pre-coma, and Hemphill (60) writes that fear of death may be consciously expressed by metrazol-treated patients. On the other hand, Cook (25), using a scale of the degree of fear exhibited by cardiazol and triazol treated patients as rated by attending nurses, concluded that the intensity of fear in 275 cases as recorded and correlated with the outcome of treatment offered no evidence for assuming that fear exerts any curative influence.

Interestingly, Dyne and Tod (30) in a comparative study of reactions to subconvulsive and convulsive doses of triazol found that non-schizophrenic mental patients reacted more prominently with fear and anxiety than did a group of "emotionally deteriorated" schizophrenics. Good (51), however, discovered that both psychoneurotic and psychotic patients showed the same fear, and he also saw no differences between these groups in their immediate post-convulsive behavior. He noted, though, that the post-convulsive disorder of dysmnnesia and disorientation was of much shorter duration in psychoneurotics than in psychotics, and this observation supported his contention that shortness of post-convulsive confusion and disorientation is a favorable prognostic sign. Löwenbach and Stainbrook (87) have put forth the opposite theory that a long post-shock disorientation after single convulsions is a good prognostic index.



Hemphill (60) postulates three psychological events in the course of convulsive treatment: (1) the realization of being treated, (2) the return to reality from the death-like state of the epileptic fit, and (3) the encounter with the environment after reestablishment of consciousness. The observation is also made that the patient is able to identify and name parts of his own body before objects in the external world, and it is therefore inferred that self-interest or awareness of the ego returns first in post-convulsive recovery. Opposed to this observation is that of Schilder (126) who states that, after metrazol convulsions, patients are able to name objects before they can designate correctly parts of their bodies. Additionally, Hemphill remarked that the use of certain emotionally descriptive adjectives may be seen as evidence of exaggerated affective attitudes existing in the immediate post-shock state. The post-convulsive subject is also described as feeling dependent and attached to persons in the environment and as assuming infantile attitudes.

Kalinowsky and Kennedy (73) have observed that post-convulsive phenomena following electric shock are surprisingly constant in each individual and they believe that convulsive and post-convulsive behavior follow the predetermined pattern of the individual and that these reactions are not influenced by the type of stimulus nor by previous medication.

*Psychoanalytic Description.* Rankine Good (51) divides the immediate post-convulsive state into four stages reminiscent of the analytic conception of sexual genesis: (1) the primary narcissistic stage, (2) the oral phase, marked by retention of gag, sucking and chewing movements, and spitting, (3) the anal phase, in which fecal smearing and coprophagia may occur, and (4) the phallic stage, during which fingering the genitals, clawing the vulva, exhibitionism, beating the genitals, and masturbation may be seen. It must be remembered, however, that these activities do not occur in every patient and that some of them are seen while the patient is operationally unconscious. Possibly as Löwenbach and Stainbrook (87) have suggested, some of these movements like sucking and chewing may be psychologically meaningless and may point to a cortical area being resistant to convulsive exhaustion. Mayer-Gross (90), discussing these oral and facial movements from a neurological point of view, suggests also that such movements may be motivated by hunger, at least in hypoglycemia. However, since sucking and chewing behavior may be seen after electroshock as well, it is unlikely that hunger is the explanation. Interestingly, Larkin (83) feels that he can prevent delayed insulin coma by stimulating oral sucking and by

"mothering" the patient by embracing and stroking him after glucose has been given to terminate the hypoglycemia. Abse (3) sees a confirmation of psychoanalytic theory in his challenging but inadequately demonstrated observation that, post-convulsively, manic-depressive patients show predominantly oral activities, while paranoid schizophrenes express primarily anal interests, and degenerative hysterics display mostly phallic behavior.

*Specific Description.* Orenstein and Schilder (102), and Schilder (126), used some of Wertheimer's original gestalt figures as adapted by L. Bender for a "visual motor gestalt perception test" in the study of gestalt organisation during and immediately after insulin reaction and in the post-convulsive metrazol period. They found disturbances in gestalt function to consist of (1) a tendency to perseveration, (2) substitution of circles and loops for points, (3) substitution of curves for angles, (4) substitution of uninterrupted lines for dotted lines, (5) changes of angles into straight lines, (6) rotation of figure parts, and (7) spatial separation of gestalt units. Stainbrook and Löwenbach (140), using these same gestalt figures over the period of post-convulsive reintegration and getting repeated drawings of a gestalt model at various time intervals after convulsion, found similar disturbances. They also noted that the earliest attempts at copying the Wertheimer designs were reactions to the whole figures, a simple loop separated in space being made for each obvious element in the model. In the recovery sequence angularity was the next feature to be correctly represented, then the units of the figure were slowly brought into proper spatial relationships, and finally, near the end of the post-convulsive recovery period, the parts of the figure were represented joined together as in the model.

These same investigators also studied the return of the writing function following convulsion. Perseveration and more tendency to completion of the task for the patients' own signatures than for other suggested phrases were noticed in the early periods. The first post-convulsive writing attempted had in spite of tremor and incoordination all the formal characteristics of the individual's ordinary handwriting. Frequently patients reverted to writing the names associated with childhood or adolescence or to the diminutive forms of their names or to a phonetically simpler spelling. Married women frequently wrote their maiden names. Printed letters occasionally appeared, and once or twice a substantive within a sentence was capitalized.

Moore (95) has compared the reactions of a patient in a mild hypoglycemic state with the pretreatment behavior, using Street's gestalt

completion test, serial subtraction of seven from 100, and the Goldstein modification of the Koh's Block Design test. He found fewer figure interpretations of the gestalt test, the appearance of finger-counting as an aid in serial seven subtraction, and inability to "abstract" the designs of the Koh's blocks. It was concluded that analytic and synthetic thinking was impaired with greater dependence upon sensory impressions, a behavior consistent with Goldstein's hypothesis of the existence of a "concrete attitude" or loss of the "categorical attitude."

Fingent, Kogan, and Schilder (34) have asked schizophrenic patients to draw the figure of the Goodenough test during the awakening from the effects of metrazol seizures or from insulin coma. They have observed that the drawings show signs of organic confusion in gestalt perception and representation. However, the typically schizophrenic handling of the test is seen when the organic confusion expressed in the drawings of the early post-convulsive stage subsides. The conclusion is recorded, therefore, that the treatment introduces a new element, the organic confusion, into the picture and that the effect of insulin and metrazol is not a direct attack upon schizophrenic structure but upon "deeper seated structures" leading to their reconstruction and reorganization. If a concomitant reorganization of the schizophrenic process takes place, it is "reflected in the gradual loosening of the schizophrenic characteristics of the drawings."

Following Angyal's (6) neurologizing assumption that the symptoms of hypoglycemia may be encompassed within six insulin-shock syndromes, (1) frontal lobe, (2) ontogenetic, (3) aphasic-amnesic, (4) static-paresthetic, (5) coenesthetic, and (6) parieto-occipital, Gyfaras (55) has observed two kinds of spontaneous drawing disturbances in hypoglycemia. One group of disturbances is considered to be frontopolar in origin with evidence of elimination of inhibition, conventions, and schemes of thought, and a general reduction and regression to a more primitive personality is seen. This inferred regression is entirely absent from a second group, (the parieto-occipital syndrome), where disturbances of drawing due to the dysfunction of the "constructive faculty" prevail without infantile and primitive personality traits, probably a kind of drawing behavior reflecting a disorder like the constructive apraxia of Kleist.

Vujić and Ristić (146) have studied disturbances of colored after-images during hypoglycemia and they report that on the average the duration of after-images is reduced by 68% and that complementary color is lost in certain cases. In 50% of the subjects there was a temporary complete loss of after-images.

Kao and Lyman (76) found that electric shock treatment abolished in an eidetic patient the prolonged, mobile transformations of the eidetic imagery. Afterwards, the eidetic responses regained their former vivid and colorful character and returned to practically the same state as before treatment was instituted.

On the basis of electromyographic and cinematographic studies of single metrazol seizures, Strauss, Landis, and Hunt (141) divided the convulsion into a first clonic stage, a tonic stage, and a second clonic phase. The second clonic phase is considered to be like the tonic phase except that interruptions in the continuity of innervation appear. The first clonic stage is different from the second clonic stage and is probably due to cortical stimulation. The tonic stage corresponds to a state of decerebrate rigidity.

Kino (79) found that the characteristics of dermatographia did not change before and after single electroshocks, but Fortuyn (37) saw alterations of dermatographia in the hypoglycemic state, consisting of a decrease in the time of latency and the appearance of a broader red line than normally present. The white area surrounding the red line became regressive or was even absent.

Kino and Thorpe (80) also described the occurrence of the grasping reflex in post-convulsive stages of electrically induced seizures and found, quite interestingly, a difference in frequency of the appearance of this reflex in acute schizophrenia and in manic-depressive psychosis. It would be desirable to have further confirmation of this observation that the expression of a neurological reflex is selectively influenced in the post-convulsive state of patients with different mental diseases.

Using a modified form of the Rorschach test presentation, Stainbrook (137) was able to assemble composite Rorschach psychograms in many cases for each five minute interval following the onset of an electroshock convulsion. He concludes that progressive increase in productivity and accuracy of form conceptualization, disappearance of perseveration and simple color-naming responses which appear in the earliest records, and the gradual appearance of FC, or form-color responses following a phase of primarily CF, or color-form answers were the main Rorschach indices of change in the post-convulsive period. Movement responses were always the last Rorschach concepts to reappear in the records.

Flescher (36) studied the extent and duration of retrograde amnesia following electric shock in 18 early schizophrenics, giving various memory tests before the shock and then again at times varying from a few minutes before shock to four to seven hours after. It was concluded that



material which does not appear spontaneously or through association or recognition at the end of this time is permanently lost. Hemphill (60) investigated the characteristics of the recall of eight pictures presented to his patients about 30 minutes before the injection of metrazol and again at about an hour after the convulsion. He writes that no patient failed to recollect having seen the pictures and that no patient remembered less than three pictures and that three patients were able to describe all the pictures. He also insists that the absence of retrograde amnesia is easily demonstrated after electrically induced convulsions.

Ten patients treated by electric convulsive therapy were taught paired word associations by Zubin and Barrera (162) before treatment, and their retention was then tested after single shocks by recall, relearning and recognition methods. A control series of associates had been learned and tests of retention given a week before treatment. They found no significant saving in the experimental series, although the learning ability was not impaired. The recall and recognition scores also showed evidence of memory loss. They concluded that shock affects material learned immediately before the convulsion more than that learned less recently. Zubin (161) also introduced interference in the form of new associations to the original words and found that such interference was accentuated after shock. It therefore appeared that electroshock disorganizes but does not destroy memory traces. Rodnick (120), publishing the results of one of the few well-devised experiments of the effect of therapeutic shock upon habit systems in the human subject, persuaded 21 schizophrenes to learn two similar but antagonistic simple habits of moving the finger either to the right or left depending upon the frequency of a tone. Twenty-four hours intervened between the learning of the first habit and the similar but opposite second habit. Patients were retested one and a half hours after metrazol shock to determine habit dominance. He found a statistically higher number of reversals to the older habit in the shock group than in a similar control group. This indicated to him that metrazol exerted a differential effect with reference to older and to more recently acquired habits.

Wiedeking (153) used tests like Bourdon's attention test and a word-association test and demonstrated in normal subjects who voluntarily submitted to Sakel's technic of inducing hypoglycemia that a parallelism existed, with small doses of insulin, between mental impairment and falling blood sugar. With high insulin doses there was a continuous drop in mental efficiency regardless of the variation in the blood sugar level.

*Subjective Description.* Wiedeking (153) reported the subjective ex-

periences of three medical students who volunteered to subject themselves to insulin-induced hypoglycemia. They described feelings of marked hunger, heaviness of the limbs and general weakness, feelings of being tired and wanting to be left alone. The mood was one of apathy. Consciousness varied in phases with "clouding." A feeling of blankness developed out of difficulty in thinking. Disorders of sensation were frequent. Entoptic phenomena and pseudo-visual hallucinations occurred. On awakening, euphoria and a sense of having been saved from danger were marked.

Gillespie (47) published the results of an inquiry into the subjective sensations of 23 mental patients undergoing metrazol treatment, but because the patients' accounts tended to be distorted by their mental condition he, himself, underwent a metrazol convulsion. He described distinctly unpleasant feelings with headache, anorexia, and malaise for many hours afterwards. There also existed some retrograde amnesia even 10 hours after the convulsion.

Watkins, Stainbrook, and Löwenbach (147) reported a subconvulsive electric shock reaction in a 25-year old normal physician in which absence of somatic and mental complaints and the presence of amnesia and disorientation were the outstanding features. Immediate post-convulsive copying of a gestalt figure was accomplished initially by making dextrad circling movements, following which the subject attempted to "close in" on the model and to draw directly on the figure, behavior similar to that seen in psychotic patients in the early post-convulsive period. Some dysmnnesia in the ability to designate concepts was evidenced by the Rorschach test. The existence of anterograde amnesia was also displayed on the Rorschach blots by the continual repeating of responses which he had already given and which he had forgotten had once been given.

Fraser and Sargant (38) have published written accounts by schizophrenics at the end of an insulin treatment course in which their subjective experiences during their illness are detailed. No attempt was made to analyze these personal records and a reading of the five typical letters published gives no knowledge of how these patients interpreted either a single insulin reaction or the total treatment.

#### OBSERVATIONS DURING AND AFTER TREATMENT

*General Description.* Polatin and Spotnitz (115), describing an ambulatory insulin shock technic with which they treated 44 schizophrenic patients with 82% showing clinical improvement, write that such improvement occurs in four progressive stages: (1) improvement in spon-

taneity, (2) greater interest in the environment, (3) dissolution of psychotic ideas, and (4) readjustment to the ordinary ways of living.

Harris (58) reports that the characteristic picture seen in schizophrenic subjects treated by cardiazol was one of euphoria and mild overactivity. The euphoria is described as causing paranoid patients to forget their grievances at least for a time. This writer also remarks that cases of catatonic stupor passed into catatonic excitement or into hebephrenia if they did not recover and that mute and inaccessible patients began to talk, that untidy patients became cleaner, and that the unoccupied began to work. Schilder (126) thought that, with metrazol, catatonic symptoms very often disappeared first while paranoid ideas frequently remained.

Friedman (40), employing metrazol, stressed the changes revealed in spontaneous verbal and written expression in 70 chronically ill patients, some of whom began to write letters after having not done so for years. This observer also felt that there soon occurred a fear of somatic organ changes in almost every case, and he saw behavior which he described as "an expression and release of previously dissociated, immature, and pathologic sexuality." Alterations in affect, increased sensitivity, and sudden impulsive-destructive reactions directed either outwardly or towards the self and occurring in previously apathetic or stuporous individuals were also noted.

Feldman, Fiero, and Hunt (33), describing suspicion, agitation, fear, and fairly well-systematized ideas of persecution in patients before treatment, saw that, following treatment, these patients displayed personality changes from the schizoid characteristics to overt, well-balanced social attitudes. However, these changes were mostly only temporary. One case, interestingly, became psychotic again with a hypomanic extraverted reaction in contrast to the pre-treatment introverted schizoid personality. Blair (13) has noted that the firmly rooted delusions of the typical paranoid schizophrenic are rarely affected by shock treatment but that delusions of a superficial and bizarre nature attached to or resulting from changes of mood may disappear. Bain (8) also had little success with metrazol therapy when there existed either fixed delusions or pronounced apathy.

Cheney, Hamilton, and Heaver (22), in treating patients with metrazol, observed that the symptoms of depressed and agitated states were supplanted by a pathological though not marked elevation of mood. Slight hypomanic states usually followed recovery from depression and manics occasionally appeared slightly depressed after electric shock therapy in the hands of Kalinowsky, Bigelow, and Brikates (72).

There is general agreement that shock therapy is most valuable in the treatment of depressive states. As Kalinowsky (70) observes, a depression can be cut short at almost any time by adequate shock treatment.

Kant, Phillips, and Stolzheise (74), presenting a description of shock treatment in schizophrenia, state that hallucinations, body sensations, and all unusual symbolic expressions are the first symptoms to disappear during treatment, but they have observed that, although the voices cease, the ideas of persecution remain, and although the patient may be no longer tormented by "electric rays," the idea of being influenced is vaguely present. Silbermann (130) has also noted that hallucinations may often disappear during treatment, and Moore (94), who discussed the maintenance treatment of chronic psychotics by electrically induced convulsions at the rate of about four to six a month, described the temporary disappearance of hallucinations. Gruenberg (54) found in treating schizophrenics with high frequency current that towards the end of treatment auditory hallucinations became subjectively less loud, less clear, and more distant. He applied this treatment with some success in alcoholic hallucinosis.

y Rosas (122) used metrazol to treat hysteria and obsessional neurosis and concluded that the most resistant symptoms were those of a compulsive nature, particularly the obsessions and the phobias. Metrazol was used, too, by Owensby (103) in the therapy of homosexuality and lesbianism and he reported six such cases as "cured" with in some cases establishment of normal sex relations and no return of homosexual desires or tendencies for as long as 18 months after treatment. Liebman (84) subjected a psychotic, transvestic male homosexual to electric shock and obtained remission of the psychosis, disappearance of the transvestism, but only some inhibition of the homosexual behavior.

Weigert (149), who is a psychoanalyst and therefore more critically resistant to overenthusiasm about shock treatment, writes that the patients are changed in emotional behavior during shock therapy for a more or less limited time without having changed their fundamental attitudes towards life-problems. But Kalinowsky (70), too, agrees that, at least in psychoneurotic depressions, although the depression is favorably treated, the neurotic attitudes remain. Varying the emphasis somewhat, Schilder (126) thought that the "psychoses and the psychotic symptoms are not forgotten" but that the individuals have changed their emotional attitudes. Berrington (12) has published a few very interesting expressions from cardiazol-treated patients which substantiate the attitude change towards less intensity in the activation of



mental content. One patient, for example, said that "It (the treatment) has held my imagination in curb. There is nothing I am hunting for."

A probing psychodynamic approach in patients undergoing metrazol therapy does not seem to have the disturbing emotional aftermath that such an analysis frequently evokes in individuals not in convulsive treatment, according to Cheney, Hamilton, and Heaver (22). Eissler (31), who was primarily interested in an analytic evaluation of metrazol treatment, felt that schizophrenic patients so treated showed, in addition to lack of self-observation, a lack of emotional depth and a withdrawal from personal contacts. He also thought that these patients' dreams had, after treatment, degenerated to expressions of predominantly simple overt wish-fulfillment such as Freud originally believed characteristic of small children. And referring to changes in the dreams of patients undergoing shock therapy, an area of research which has received but little attention, Boss (18) believes that if there is no change in the dream-content of patients being treated by convulsive therapy the prognosis is poor.

Moore (94), treating some patients by a maintenance regime of four to six convulsions monthly for in some cases as long as two years, insists that clinical observation of these individuals shows no deterioration in personality or intelligence. This observation, however, is not objectively substantiated. Neymann, Urse, Madden, and Countryman (100) also saw no dementia or flattening of the personality among the patients of their recovered group of schizophrenics, manic-depressives, and chronic alcoholics subjected to electroshock therapy.

Kalinowsky (71) believes that all patients undergoing electric convulsive therapy show an early impairment of memory as well as emotional disturbances. He does not feel, however, that learning is grossly affected and cites an interesting case of a refugee physician who was treated for depression until he was deeply confused and had confirmed Petri's rule by losing his ability to speak English. Nevertheless, a few months later he passed a state board medical examination. Smith, Hastings, and Hughes (133) mention memory changes as always occurring to some degree with electroshock therapy, but they write that "these memory defects do not seem to be permanent." Schilder (126), too, thinks that the amnesia seen after metrazol treatment completely clears.

Whatever may be the reports concerning the complete disappearance of shock-induced memory impairment, no one who has talked to patients who have undergone electroshock treatment can doubt that there is a considerable amount of experience surrounding and during

the course of treatment which remains permanently inaccessible to memory. Quite parenthetically, it is interesting to note that Ingalls (68) successfully treated a case of hysterical amnesia by metrazol-induced convulsions. After the fourth convulsion, his patient is reported to have regained complete memory of his past.

Harris (58) remarks that the majority of his cases gained weight but that the weight gain was independent of mental changes. There occurred a marked weight increase even in patients who showed no recovery. Reports of gain in weight are perhaps the most consistent and universal treatment results described by users of shock therapy, of whatever type, and psychosurgery.

Early in the history of insulin therapy considerable attention was paid to the acute psychotic picture sometimes occurring during treatment. Sakel (125) called this the *activated psychosis*. However, as Kalinowsky (71) has observed, "all the known varieties of symptomatic psychoses occurring in infectious and toxic diseases can be seen in a long course of electric shock treatments." These states are reversible and, as Glueck and Ackerman (50) point out, these acute psychotic episodes are not like actual psychoses in that they are (1) acute and unstable, and because (2) the emotional display is genuine and reflects depth, (3) the mental content is chaotic, (4) the patient makes more direct demands upon the environment with less recourse to veiled symbolic expression, (5) the patient reacts to frustration of demands more positively and energetically and with more direct expression of hostility, and (6) the patient exhibits primitive infantile but strong transference.

*Specific Description.* Much of the early psychologic research in shock therapy centered about attempts to arrive at factors which might afford a basis for the selection of patients for successful therapy. Bolles, Rosen, and Landis (17), using the Vigotsky, Weigl, and BRL sorting tests on 19 schizophrenes treated by insulin, concluded that the patients whose performances were superior before treatment showed most improvement and that the patients with poor pre-treatment performance showed little or no improvement.

Skottowe (132) felt that patients who showed what he called *dys-symbole*, ("a state of mind which manifests itself by the inability of the patient to formulate his conceptual thoughts upon personal topics or to discriminate the gradation of his emotions in language which is intelligible to others . . . notwithstanding that he may be in a state of clear consciousness"), do not respond favorably to shock therapy. Thomas (143), believing *dys-symbole* to be pathognomic of true schizophrenia, observes that in 32 cases which he treated with either insulin or cardi-

azol, no case showing unequivocal signs of *dys-symbolie* made a good recovery, nor did any recovered case at any time show evidence of this kind of thinking.

Piotrowski (110), using the Rorschach test, felt that, on the whole, the difference in pre- and post-treatment records paralleled the clinical improvement. This improvement was manifested on the Rorschach test by (1) improvement in speed and ease of answering, (2) better logical content of responses and less mixing of description and interpretation, (3) an increase in the number and quality of movement responses, (4) an increase in the number and percentage of form-color concepts, (5) an increase in percentage of sharply perceived forms, and (6) good percepts and integrating capacity. Signs of predictive value for good prognosis were devised (112) and described as (1) variety, indicating no concepts used more than twice, (2) generic term, referring to patient's attention to logical hierarchy, (3) evidence, or self-critical evaluation of responses for "adequacy of fit," (4) color response, or at least one color interpretation except simple color-naming, (5) indirect color, or evidence of attention to color-areas, and (6) demurring, or holding back one or more responses. Elsewhere, Piotrowski (111) has reported that patients who give both color and human movement responses to the ink-blot tests have the greatest chance of benefiting from insulin therapy. As seen in the performance on the *concrete-abstract* tests, Piotrowski also concludes that on the pre-treatment records the patients who improved with shock therapy were functioning on "a higher intellectual and emotional level" than was the pre-treatment unimproved group.

Halpern (57) studied with the Rorschach inkblots 17 schizophrenes before and after insulin treatment and found that the improved and unimproved differed reliably in that the improved group gave greater number of responses, five times as many movement responses, more color responses, and a greater number of human concepts. Eisner and Orbison (31) also gave the Rorschach test before and after metrazol therapy and decided that the patients who benefited were "more emotionally inhibited, more constricted, and more socially withdrawn" than those who did not benefit. Moreover, these investigators felt that no matter what clinical improvement occurred these patients "remained unequivocally schizophrenic after metrazol."

Morris (96) administered the Rorschach test to 41 patients undergoing metrazol therapy and applied the chi square test for the validity of his differential signs and concluded that patients tending to remain unimproved gave 15% or more anatomy responses and more than two

color-form concepts and less than 70% good form responses in their pre-treatment records.

Kenyon, Rappaport, and Lozoff (77) used the Rorschach test, the Babcock deterioration index, and the Szondi test before and after metrazol treatment of three paretics. To the Rorschach blots the paretics reacted with less total color expression following treatment. The Babcock test scores improved with clinical improvement, but, of course, marked deterioration existed even in the fever- and metrazol-improved patients.

Kisker (81) indicated that the pre-treatment Rorschach picture of insulin- or metrazol-recovered patients was characterized by lack of concentration, numerous incidental remarks, good perception accompanied by uncontrolled, extensive associations, and an unevenness of performance. He also observed a falling off of the mean M or movement score after the start of pharmacotherapy and the appearance of confusion after 20-30 shock days. Like almost all users of the Rorschach blots in the study of shock-remitted psychotics, he noticed that some patients with good clinical improvement still showed psychotic characteristics in their Rorschach performance. Weil (150), too, writes that psychotic features appear in the Rorschach situation even after an insulin-treated patient appears clinically cured.

Using standard intelligence tests, Wittman (156) found that test scores increase progressively after metrazol treatment begins until the ninth convulsion when they gradually decrease. She and Russell also noted that the intellectual performance improved in interest, attention, and social responsiveness (157).

Wechsler, Halpern, and Jaros (148) made a comparative psychometric study of mental efficiency of schizophrenic patients immediately before and after insulin treatment. The performance on Wechsler's vocational interest blank, a test of counting by three's, naming words in three minutes, and a similarities and a directions test gave a correlation of .73 and a correspondence of 87% with a clinical appraisal of the patients' condition six to 18 months after the end of treatment. The increase in the number of "liked" occupations on the vocational interest list was particularly marked in the improved patients. Their analysis also suggested that certain patients may be harmed insofar as test performance after treatment is concerned. McNeel, Dowan, Myers, Proctor, and Goodwin (91) used a large battery of psychometric tests before, during, and after insulin therapy and concluded that a fair correlation existed between clinical psychiatric rating of post-treatment status and the psychometric rating. Luborsky (88), too, gave a battery



of 22 psychometric tests to 12 patients before, during, and after treatment by electroshock. He concluded that the profiles of the schizophrenic patients showed a general decrease in test scores from the line of zero change in the *before-during* comparison. This decrease was not as marked in the *before-after* contrast. The profiles of the depressive patients showed large score increases in the *before-during* comparison and further increase in the *before-after* contrast.

Sherman, Mergener, and Levitan (128) employed several memory tests consisting of designs, directions, and a reading paragraph and found a slight improvement in all tests after the conclusion of electroshock treatment.

The tests of Zucker and Herbert were used by del Pino (27) with schizophrenic patients during cardiazol treatment. He described improvement as being reflected in quicker appearance of images, easier representation of scenes, and in the absence of the deviation or replacement by other images such as occurred before treatment.

O'Connell and Penrose (101) studied the psychomotor efficiency in 30 patients treated with metrazol, quantifying the reaction time to an auditory stimulus, the tapping rate, and the strength of grip, and they concluded that in patients who showed before treatment marked incompetence in the tests, whether due to stupor or to agitation, improvement was greatest. They also found that the increase in the tapping speed was greatest in the early phase of treatment and that the amount of improvement decreased steadily as the number of convulsions increased. Subconvulsive doses of metrazol did not contribute appreciably to improvement in tapping scores.

Solomon, Darrow, and Blaurock (134) measured the blood pressure and electrodermal response during standardized interview situations before and after insulin and metrazol therapy and found, following pharmacologic treatment with clinical recovery, that in two groups of patients (one group showing pre-treatment psychological resistance to the examiner or to the test-situation and the other evidencing resistance involving perplexity in discussing or the evading of emotional problems, and all of whom gave large blood pressure reactions and small galvanic responses to several ideational stimuli in the interview), the autonomic responses were altered to show an unchanged blood pressure and larger palmar galvanic changes. Patients whose pre-treatment resistant attitudes persisted and who were unchanged by treatment revealed no change in the character of their autonomic responses. Patients who were overtly passive and cooperative both before and after treatment showed both blood pressure and galvanic

responses of large amplitude before treatment and these responses remained unchanged after shock therapy whether or not recovery took place. They concluded that improvement or recovery involves a decrease in inhibitory effects and favors increased sympathetic reactions in combination with and partially balanced by greater cholinergic response to ideational stimuli.

#### INTERPRETATIONS OF THE PSYCHOSIS-MODIFYING EFFECTS

*Non-analytic Theory.* The lack of enthusiasm in contemporary psychiatry for the various systems of psychologic description is amply confirmed by the occurrence of an almost complete adherence to psychoanalytic concepts in the reports of psychiatrists describing the effects of shock therapy upon psychotic behavior. Not all of these psychiatrists are psychoanalysts in the formal sense, and, hence, some of the writing is hesitant and tentative in the use of analytic constructs. However, the psychoanalysts, themselves, have been generally very keen observers of what happens to the shock-treated personality, but their descriptions are so admixed with analytic interpretations that in many cases the facts of behavior cannot be distinguished from opinions about the facts.

Nevertheless, various non-psychoanalytic interpretations of the process of symptom-remission following shock procedures have been put forth. Otto Pötzl, who made possible the easy introduction into psychiatry of Sakel's treatment, believed that the symptomatic disappearance of the psychosis was due to the disturbances of consciousness and to the associated impairment of memory. Myerson (99) also attributes recovery to the memory difficulty. He thinks that the "mechanism of improvement and recovery seems to be to knock out the brain and reduce the higher activities, to impair the memory, and thus the newer acquisition of mind, namely the pathological state, is forgotten. As the brain recovers, the well-established trends—those which are relatively normal—come back, but the incubus of more recent evolution and with less roots—so to speak—of thinking, feeling, and doing, remains away."

The neurological descriptions of the interruption of "pathways" and of either the creation or abolition of disturbances in cortical neural "connections" and of the readjustment of the neuronal "shortcircuiting" supposedly associated with the abnormal behavior constitute some of the neurologic attempts to explain the remissions from psychosis observed following shock therapy.

Referring to the work of Löwenbach and the writer, Fulton (43)

considers electroshock treatment as a *functional ablation* of the frontal areas and links this kind of therapy with psychosurgery as, indeed, Freeman had done earlier. The psychological interpretation of the efficacy of electric shock treatment from this point of view is that this *functional ablation* of the frontal cortical areas removes some of the influence of these areas from active participation in behavior and thus psychologic projection into the future with the attendant anxiety is no longer easily possible for the subject.

Fear of the convulsive experience, particularly of the convulsions evoked by agents other than electricity, has been considered as an important motivation for the patient to control psychotic behavior in favor of more adequate reality-testing.

*Psychoanalytic Theory.* The psychoanalysts have generally stressed the punishment aspects of shock treatment, holding that convulsions may discharge energy derived from inwardly-directed destructive tendencies, particularly manifested in the depressed states, so that either the expression and relatively guiltless acceptance of hostility may then be possible for the patient or so that a relative increase in the strength of the life-instincts may occur. Weigert (149), for example, writes that the "cruelty of the super-ego is replaced by a sadistic attack on the part of reality" and so, by discharging the self-destructive urges from the inner conflicts, reality becomes again the object of libidinal cathexis.

Then, too, the primitive transference situation many times established following convulsions may give reassurance to the patient, as Wilson (155) indicates, that the good and beloved parent figures are real and that the hate and death wishes regressively generated around the primal negative super-ego images are not triumphant over the love-giving representations. Bychowski (20) also observes that the pathologic ego which has been distorted by the infantile desires of the id is partly destroyed or weakened by treatment. The resistance against reality is therefore decreased and the readiness for a positive transference is increased.

Abse (1, 2, 3) believes that the treatment situation evokes objective anxiety and that this threat of external danger orients the ego to the current reality-situation and results in an anxiety-motivated repression of the psychotic content so that, in schizophrenia at least, the patient is no longer "at the mercy of his complexes." The reestablishment of the functional supremacy of the ego-complex is thus achieved as a result of these repeated danger situations and the concomitant heightening of somatic sensation involved in the convulsive experience. In an

article by Schilder (126) the euphoria commonly seen following shock treatment has been ascribed to the overcoming of the death threat of the convulsion with the consequent feeling of a hypomanic joy of rebirth.

Almost all analysts are agreed that the aims of psychoanalytic therapy are in direct opposition to the apparent results achieved by shock therapy. The aim of psychoanalysis is seen either as the mitigation, and not the mere replacement in kind or even augmentation, of the cruelly primitive super-ego so that the patient may be able to bear the necessary burdens of reality, or psychoanalysis is directed to the weakening of repression and not to the reinforcement of the repressive mechanism of defense which the analysts see as the result of convulsion therapy.

#### ANIMAL EXPERIMENTATION

The experimental quest for an adequate theory which would make rational the empiricism of convulsive therapy as used in psychiatric treatment is probably best oriented around human subjects. Certainly there is now no lack of a human experimental population with which to work. Nevertheless, psychologists and other workers interested in the rationale of convulsive treatment will want to study by animal as well as by human experimentation the complex of psychologic happening which is included in the effects of artificially induced convulsions. The results of many such controlled investigations are in existence concerning the behavior of animals following convulsions induced by various procedures. Most of these studies refer primarily to the experimental effects upon learning and upon the determinants and cognitive structuring of habits.

In a study of the influence of metrazol convulsions upon maze-learning, Bunch and Mueller (19) found no significant differential effect upon the learning by rats of a 14 unit multiple T-maze. Heron and Carlson (63) substantially confirmed this observation, although Loken (85) indicated that both maze time and errors may be increased following these pharmacologically induced seizures. Stainbrook (136) found that the relearning maze time was greatly lengthened following a long series of electroshock convulsions although the relearning errors were no greater than in a control group. The differentiating ability of the dog in a conditioning situation is reported to be disturbed immediately following the administration of metrazol, but, according to Rosen and Gantt (123), discriminatory efficiency tends to return with recovery from the effects of the convulsion. Kessler and Gellhorn (78)



have shown that convulsions reactivate in the rat a conditioned response previously inhibited by non-reinforcement. Rose, Tainton-Pottberg, and Anderson (121) administered a series of insulin shocks to a well-trained sheep in which a conditioned reflex had been standardized in tests extending over a period of seven years. They found that, following the hypoglycemic treatment, the conditioned reflex which had been almost entirely absent for one year reappeared with abnormal vigor. Interval leg movements, associated ordinarily with experimental neurotic behavior, appeared for the first time in the history of the animal and became a relatively permanent part of the animal's reaction to the experimental situation. Page (104) and Stainbrook (135) have also observed signs of abnormal behavior in animals following electroshock convulsions, and Stainbrook and de Jong (138) saw reactions of the rat indistinguishable from the state of "experimental catatonia" which ensued immediately after the cessation of an electroshock convulsion.

Riess and Berman (117), using a relatively complex and a simpler maze, concluded that insulin shock has a greater disintegrating effect on a less well-fixed habit than upon one of greater fixation, that the disintegrative effect is greater on the learning retention of a longer and more difficult maze than on a shorter and easier maze, and that hypoglycemic treatment has a disintegrating effect upon the initial acquisition of a maze habit. Siegel (129), however, concludes that "the retention of a 'barely learned' simple response in the rat is not affected by a series of electroshock convulsions," and he similarly reports that the ability to learn a simple habit is also not affected by such convulsions. Stainbrook and Löwenbach (139) have indicated that in a simple water-maze a long series of either electroshock or noise-fright\* convulsions does not alter the maze behavior of the rat insofar as error scores are concerned; time scores, however, are significantly changed.

All experimenters with convulsive technics who use animals in controlled learning situations must take into account the change in the behavior of the animal after it has been "shocked" a few times. The increase in maze time following such procedures is very probably a reflection of this behavior change. The conclusions of Riess and Berman, particularly, being one of the few reports showing a permanent shock-destructive effect upon already learned maze habits, must be weighed by this consideration, especially since they saw that insulin had an unfavorable effect upon the initial acquisition of the maze habit

\* Noise-fright is used to describe the reactions in the rat which are called *audiogenic*, *audiepileptic*, or *abnormal behavior* by various other investigators.

as well as upon learning retention. The change in maze behavior reported in the other situations measuring learning retention may have been due primarily to this general behavior change and not to a direct "shock" effect upon the habit system. Certainly, in a water-maze with constant escape motivation, it is impossible to significantly alter a well-trained maze habit, insofar as error scores are concerned, beyond an immediate post-convulsive period of shock effect varying with the spacing and number of convulsions given.

Obviously, in all these studies on the behavior of animals after convulsive treatment, both time and error scores should be used in quantifying the results and a description of the total behavior of the animals with a few simple operational specifications of such behavior should be included.

The field of animal experimentation either with reference to the effects of shock upon habits in the elaboration of a theory of shock therapy or in relation to the use of convulsions in experiments designed to test various postulates of the theories of learning is currently quite open, and experiments of this nature should be extraordinarily fruitful for the pursuit of both objectives.

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## THE USE OF THE WECHSLER-BELLEVUE SCALES: A SUPPLEMENT

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In the July, 1945 issue of the *Psychological Bulletin*, Rabin (17) described in succinct fashion certain of the findings that have come about with the use of the Wechsler-Bellevue Scales with normal and abnormal persons. He stated that the review was "... an attempt to coordinate and summarize the findings to date ..." (17, p. 410). On examination of the literature through 1944 it appears that fifteen research studies and clinical applications have not been discussed. One of these studies appeared in 1941, two in 1942, five in 1943, and seven in 1944. The purpose of this note is to supplement and to amplify the material given in the previous article.\* The material is organized following Rabin's plan of presentation with the addition of two sections, one on use in the Armed Services, and the other on the rationale of the various subtests. Assuming accuracy of report, specific studies previously presented will not again be examined with the exception of certain studies of Lewinski about which confusion might otherwise exist.

### COMPARISON WITH OTHER TESTS

Five additional studies which present comparisons between the Wechsler-Bellevue Scales and other measures appeared prior to 1945. The relationship between the Herring Revision of the Binet-Simon Tests and the Verbal Scale was examined in two studies by Lewinski (10, 14), who used as subjects 100 recruits referred to a Naval clinic because of suspected mental retardation. Scale A and Scale B of the Herring Revision showed correlations of .65 and .64 respectively with the Verbal Scale notwithstanding a limited intellectual range. For the two Herring scales the difference in IQ points from the Verbal Scale was less than 5, in 55 and 40 per cent respectively and more than 15, in 5 and 12 per cent. The four mean IQ's were in the neighborhood of seventy, with the Wechsler IQ's higher in 58 and 76 per cent of the cases. Goldfarb (5) administered the Revised Stanford-Binet, Form L, to 60 superior foster home children between the ages of 11 and 17 and found the IQ correlation to be .86, .80, and .67 with the full, Verbal, and Performance Scales respectively. In 70 per cent of the cases the Stanford IQ was higher than the Wechsler-Bellevue IQ with

\* The bibliography is limited to studies appearing in 1944 or earlier years.

the mean of the former being almost 5 points higher than that of the IQ on the full Wechsler-Bellevue Scale. Goldfarb concluded that the low scores on the Wechsler-Bellevue make the test relatively ineffective in discriminating among a group of superior adolescents. A further study comparing Stanford-Binet and Wechsler-Bellevue scores of negro and white criminals by Maizlish (16) has as yet appeared only in abstract form. Incidental to a comparison of narcotic addicts and matched hospital attendants, Brown and Partington (2) calculated for the entire group of eighty-four the correlations between the Wechsler-Bellevue and other tests. Among these were representative correlations of .66 with the USPHS Number Series Completion Test, .63 with the USPHS Paper Form Boards, .60 with the USPHS Maze Test, .59 with the Ferguson Formboards, .48 with the Knox Cubes, and .41 with Healy Picture Completion Test II.

Rabin, in his subsection on clinical status (17, p. 412), confuses two studies by Lewinski. The Lewinski study cited (12) is concerned with a comparison of the Kent Oral Emergency Test and the Verbal Scale. This study is summarized in Rabin's subsection on correlation with other tests (17, p. 411). Contrary to Rabin's statement, this article contains no mention of performance on individual subtests. The reference not cited is a study of the variability of the subtests of the Verbal Scale (11) to be discussed later. Rabin's quotation in regard to Digit Span (17, p. 412) is found in this latter study. Still another study by Lewinski (13) omitted by Rabin is relevant to the problem of clinical status. The Wechsler Verbal Scale was given to 451 Naval recruits referred to a neuropsychiatric clinic for an examination to determine their fitness for duty. The recruits' scores were grouped on the basis of Verbal Scale IQ into categories of Normal, Dull Normal, Borderline, and Mentally Deficient. Critical ratios between the mean scores of these groups for each individual subtest were calculated, and all found to show statistically significant differences between immediately adjacent groups. It will be noted that this study went beyond that of Wechsler, Israel and Balinsky (22) in that it investigated the discriminatory value of the verbal subtests at the four levels of intelligence mentioned.

In resume, it would appear that fairly high correlations are found between the Wechsler-Bellevue Scales and verbal measures of intelligence but that the correlations with performance type scales are somewhat lower, although still substantial. The trend reported by Rabin of relatively higher Wechsler-Bellevue IQ's for duller subjects and relatively lower ones for brighter subjects is substantiated. Adequate dis-

crimination of each of the verbal subtests at different intellectual levels is obtained.

#### TEST RESULTS AND CHARACTERISTIC PATTERNS OF SPECIAL GROUPS

The studies reported in this section are concerned with the varying scores obtained on the subtests of the Wechsler-Bellevue Scale in various personality and behavior disorders. As Rapaport *et al.* point out, "one cannot expect every case within the same nosological group to have identical test-patterns; but one can expect that in any nosological group, a large share of cases will have, as a reflection of the disorder, similar impairments of certain intellectual functions, and these impairments will become evident in the intelligence test scores" (19, p. 23).

The intertest variability of the scores of 158 mentally deficient and 189 borderline defective Naval recruits on the Verbal Scale was reported by Lewinski (11). In the mentally defective group the mean score on the Arithmetic subtest was significantly lower and Comprehension and Similarities significantly higher than the other subtests. The comparatively low score on Arithmetic corroborates the findings of Magaret and Wright (15) and Wechsler *et al.* (22). Cleveland and Dysinger (3) studied the Wechsler-Bellevue scores of 20 institutionalized senile patients with a mean age of 75.1 years. Many of the subjects made low or zero scores on the Performance subtests, which confirms the finding reported by Rabin (17) of a more consistent trend to decline in Performance subtest scores with age. Their results on the individual subtests disagree with those of Wechsler (21) since they found results contrary to his claim that Similarities did not "hold up" with advanced age, and that Object Assembly did do so. Unfortunately, only an abstract is as yet available of the study of Van Vorst (20) who used as subjects a relatively small number of delinquent boys who had been diagnosed as psychopathic personalities. He states that the results "... do not appear to support the claims . . . to an extent which would justify defining any characteristic response pattern for the psychopathic personality."

The most extensive and detailed study of scatter of Wechsler-Bellevue scores yet to appear is that of Rapaport and his collaborators (19). Their nosological groups included paranoid and unclassified schizophrenics, both further subdivided into acute, chronic and deteriorated cases; simple schizophrenics; paranoid conditions; preschizophrenics divided into coarctated and over-ideational; depressives further subdivided into psychotic, involutional, severe neurotics and

neurotics; hysterics; anxious and depressed neurotics; mixed neurotics; obsessive compulsives and neurasthenics. In addition they used a control group further subdivided into smaller groups on the basis of trends in the direction of maladjustment exhibited. Although including a study of mean scatter and of the presence of extremely high or low weighted scores, principal attention was focussed upon the deviation of the scores from the level of the vocabulary score. Extremely detailed findings on each of the nosological groups prevent any adequate short description. A quotation from one of their summarizations will illustrate both their findings and their rationale of these findings.

The deteriorated unclassified schizophrenics show extreme impairment on almost all of the subtests. Comprehension and Digit Span are greatly impaired; Arithmetic is even worse; and worst of all are Picture Arrangement, Picture Completion, and Digit Symbol. In striking contrast, they show relative efficiency on Information, Object Assembly, and, to a lesser extent, Similarities and Block Design. In general, the scatter of the Deteriorated group may serve as an exaggerated representation of the schizophrenic pattern: impairment of judgment (Comprehension), concentration (Arithmetic, Picture Completion, Digit Symbol), and planning ability and anticipation (Picture Arrangement).

#### THE RATIONALE OF THE SUBTESTS

Very little attention has been paid to the psychological functions tapped by the subtests except in the formulations of Wechsler (21) and Rapaport *et al.* (19). The latter disagree with Wechsler on the bipartite classification into verbal and performance subtests. The basis for their difference of opinion is clinical experience, theoretical considerations and statistical findings, although the latter is limited to inspection of mean scores and standard deviations on each of the subtests for 261 clinical and control cases. They conclude that both the Verbal and Performance Scales should be subdivided into two parts, all four "groups" differing one from the other in mean score and standard deviation. Vocabulary, Information, Similarities, and Comprehension are designated as the *Essentially Verbal* group since their common feature is the requirement of verbal presentation and response. The remaining two subtests of this scale, Digit Span and Arithmetic, are referred to as the *Attention and Concentration* group because of their presumed relationship to these functions. Verbalization is considered merely to be the form of communication. The *Visual-motor Coordination* group is so characterized because performance on the subtests, Object Assembly, Block Design, and Digit Symbol is said to depend upon coordinated visual-motor activity. The *Visual Organization*



group, Picture Arrangement and Picture Completion, requires no essential motor activity, but instead demands this function. Each subtest is also considered as calling into play certain functions or set of functions. For example, Digit Span is considered primarily a measure of attention, Similarities a measure of concept formation, and Arithmetic a measure of concentration. In many of their formulations they differ sharply with Wechsler (21) who considers, for example, that Digit Span is a measure of memory as well as attention, and that Arithmetic is a measure of reasoning.

#### USE IN THE ARMED SERVICES

The Wechsler-Bellevue Scale has been used extensively in the Armed Services (4, 7, 8, 10, 11, 12, 13, 14). In fact, as Hunt *et al.* say in reference to the detection of feeble-mindedness it "... has been adopted as the standard test for such use in the Navy" (8, p. 478). Both Hunt *et al.* (8), and Hildreth *et al.* (7) describing the psychometric procedures used at Naval Training Centers, give considerable prominence to the Wechsler-Bellevue Scale. As early as 1941, Rapaport (18) suggested a possible use of items from the Wechsler-Bellevue in examining selective service registrants suspected of feeble-mindedness. In three case histories with implications in regard to military selection, Knight *et al.* (9) described the use of Wechsler-Bellevue findings along with those from a variety of other measures. The Army has in recent years used a revision and extension of the Wechsler-Bellevue Scales, the Wechsler Mental Ability Scale (23). As described in Greenwood *et al.* (6), it consists of sixteen subtests, seven verbal and nine non-verbal or performance tests. It is so standardized that a selection of a lesser number of subtests appropriate to the situation may be made. Presumably in the immediate future we may expect to see many research studies appearing which use this presently restricted measure.

#### SUMMARY AND SUGGESTIONS

That the results of studies reported here would in some measure coincide with the results of those previously summarized is hardly surprising. A summarization showing considerable agreement with that of Rabin is natural, not to say inevitable. Repetition is obviously unnecessary. Therefore, a rounded summary is not attempted. Instead certain summary statements or cautions unreported previously will be stressed, based upon studies in both reviews. There are, however, certain points of disagreement of emphasis which should be mentioned.

As part of the first sentence of his summary Rabin states, "... the

Wechsler-Bellevue Scales . . . have supplanted some time-honored diagnostic tools" (17, p. 419). This rather broad and undocumented statement is open to question. It would perhaps be more correct to say that in the testing of adults they have *supplemented* other diagnostic devices such as the Stanford and Revised Stanford-Binet, the Babcock Test, and the Arthur Performance Scale, not to mention the whole array of less directly comparable measures which modern clinical procedure affords. In this connection it might appear to many that the dismissal of age-level intelligence tests as "hotch-potch scales" (17, p. 413) is too cavalier a treatment of such measures as the Stanford and the Revised Stanford-Binet Tests. He goes on to say in regard to the Wechsler-Bellevue " . . . that it tends to differentiate better than other measures between the dull and feebleminded" (17, p. 419). It may be this will prove to be the case; as yet the evidence on the matter is hardly conclusive. For example, the one study described by him on the relative merits of Stanford-Binet and the Wechsler-Bellevue Scale in clinical effectiveness as an aid in the diagnosis of mental deficiency was performed at the Bellevue Hospital (1). Although tempered by the social and psychiatric data, there was a possible predisposition on the part of the psychologists to urge, and the psychiatrists to accept, as more valid the findings of a scale developed at their own hospital. For a second group in the same study this factor could not operate since the Wechsler-Bellevue was given to the subjects two or three years after the recommendation for commitment or non-commitment was made. For the 36 cases on which both measures were available the biserial correlation coefficient for the Wechsler-Bellevue was  $.720 \pm .086$  while that for the Stanford-Binet was  $.611 \pm .103$ . Presumably the difference between these correlations is statistically non-significant.

The differences of opinion in regard to rationale between Wechsler and Rapaport are both encouraging and disconcerting. Encouraging, because they show that attention is being given to the meaning of the subtest scores obtained; and disconcerting, because they show that much remains to be done before a thoroughgoing rationale for the various subtest scores is established. Some might find the emphasis on a pattern of "function" a retrogressive trend in clinical psychology smacking too much of a faculty concept of mental organization. Further experience, clinical, experimental and statistical, will eventually allow more final judgments to be drawn. Studies showing whether scores on the subtests are correlated with successful performance in other situations which require the same functions seem indicated. Factor studies are also applicable. There is no doubt, even today, that individual

clinical use seems to become more meaningful upon application of some conception of rationale. Enough evidence exists to show that a pattern of variations in scatter on the Wechsler-Bellevue Scale, in part, is produced by a pattern of psychopathy, but it must be remembered that even if the subtests were pure "functional entities," differing educational backgrounds, and cultural environments, unrelated to psychopathy would tend to blur the findings making the patterns indicative rather than invariably diagnostic. The Wechsler-Bellevue Scale is a valuable clinical instrument but it does not allow the substitution of a list of diagnostic signs for clinical acumen.

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## PSYCHOLOGY FOR THE ARMED SERVICES

A SPECIAL REVIEW\*

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This is a third book published under the sponsorship of the Emergency Committee of the National Research Council, following as it does the highly-successful *Psychology for the Fighting Man* and *Psychology for the Returning Serviceman*. The original manuscript was drafted by the summer of 1944 under the general editorship of Boring, and was revised in the course of the fall and winter by other psychologists and by military experts, including Colonel Joseph I. Greene and Colonel Edward L. Munson, as well as by the Science Service expert, Miss Marjorie Van de Water.

The present book is somewhat more factual and less advice-giving than the *Psychology for the Fighting Man* and considerably so than the *Psychology for the Returning Serviceman*. It "is intended as a textbook written on the college level, but also as a book in which the military and naval applications of psychological principles and the basic principles themselves are more fully developed than in the earlier book. It was believed that a single book might be equally useful as a textbook and handbook of psychology for general use by members of the Armed Services, not simply for instruction but also for individual reading and reference" (p. ix). In organization this book follows that of *Psychology for the Fighting Man*, almost chapter by chapter. The first ten chapters, largely on sensory functions of military value, present practically the same contents, with here and there some more details and often a rephrasing of passages to decrease their newspaper and increase their textbook character, without sacrifice in clarity or readability. The same is true for some of the remaining of the twenty-four chapters. Of those chapters receiving much fuller or even reorganized treatment, one should mention especially those on *Selection of Men* (chiefly by Bingham and Harrell), on *Learning* (Seidenfeld), on *Personal Adjustment* (Hunt, Abrams, Doll, and Seidenfeld), on *Assessing Opinion and Discovering Facts* (Edwards); and there is an added chapter on *Differences Among the Peoples of the World* (Klineberg, Child, and Canady).

Certain questions naturally arise. The first that psychologists ask is: Is the material sound, scientifically and professionally? A few hasty assertions may be found in it. Examples: "It is doubtful that the

\* COMMITTEE OF THE NATIONAL RESEARCH COUNCIL. *Psychology for the armed services*. Washington: The Infantry Journal, 1945. Pp. 519.

muscles outside the eyeballs, the muscles that move the eyes, very often cause fatigue" (p. 33), "An after-dinner cup of coffee never kept any man awake because of the caffeine in it" (p. 222), and, "There is no evidence that the sexual act in animals is learned" (p. 398); but such assertions are almost inevitable in a book pitched at the level of this one—and these in particular are perhaps not to be disproven as yet by references to experimental data. Then there are certain misleading manners of statement that are likely to falsify the reader's understanding of psychological phenomena. Examples: "The brain of the man who owns the retina sorts the colors out into objects" (p. 74), "Attention picks out one set of impressions and puts them together into one object" (p. 75), or, "It was discovered that they [rats] have mental maps" (p. 157). But over against such concessions to the non-technical reader there are warnings against too hasty conclusions, as in the sentence, "The experimental results on this matter [of heavy smokers and their supposed need for more rapid breathing to get their needed amount of oxygen] are not, however, conclusive" (p. 219). And it can certainly be said that for factual soundness, *Psychology for the Armed Services* does not disappoint the expectations originally engendered by inspection of the reassuring list of collaborators.

The book before us is explicitly intended to serve as a textbook. Is it, then, teachable? That the intent is realizable is obvious enough. Each chapter and topic is organized to high degrees, with frequent employment of centerheads and sideheads that suggest didactic "points" for the student to set down and for the instructor to use as a skeleton for his presentations. Transitions from chapter to chapter are smooth enough. Not least important are the fairly generous illustrations and even a few simple tables, of wide range, whether taken from technical psychological journals or from supplies of military photographs—all of these being admirably apposite, and introduced to inform more than to entertain. The references appended to each chapter are well-selected and should be of real help to teacher and student.

Of recent years scientists have become more aware than formerly of the great gap between technical knowledge for the scientist and popular knowledge for the layman, and the urgency of building bridges between the two. To select those developments in scientific knowledge that are most relevant to human problems, to reorganize their statement into forms both understandable and interesting to the layman, and to make these cheaply available, is no mean achievement. And in the present book, together with *Psychology for the Fighting Man*, psychologists have before them effective examples of how the job can be done, and at two levels.

As in the *Psychology for the Fighting Man*, over a fourth of this book is devoted to the study of sensory and sense-perceptual processes. It seems not unlikely that that is fair reflection of the important role that seeing, hearing, smelling, and equilibration play in the combat activities of servicemen. But that it should be taken as pointing an error in the allotting of less space to those functions in textbooks of more general psychology, need not follow, as some may suppose (2). Indeed, another recent book on military psychology (3) allots only about 10 per cent to them.

*Psychology for the Armed Services* is easily distinguished from two other recent books by American psychologists for use in military instruction. Its scope is much more inclusive than is that of the Pennington, Hough, and Case book (4); and by the same token it is addressed to a wider audience, including both commissioned and enlisted men. On the other hand, its avenue of approach is distinctly different from that of Meier's (3) in that psychological rubrics determine its division into chapters, whereas discussions in the latter text take off from military problems and phases. The present work could be used in combination with either or both of the others mentioned.

The cooperative sponsoring of this book and its predecessors, both in the writing and in the distribution, by military and by scientific authorities, is a hopeful augury for applied psychology.

As a bookmaking job—paper stock, type, format, and binding—*Psychology for the Armed Services* is excellent.

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## THE EFFECTS OF SCHOOLING UPON IQ

### A NOTE ON LORGE'S ARTICLE

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In a recent publication,<sup>1</sup> Dr. Irving Lorge has presented data which have been widely hailed as demonstrating the thesis that "education increases the IQ." Such an interpretation of his study is entirely fallacious, and it would be most unfortunate if it were generally accepted. Lorge has, indeed, shown what psychologists have long known, namely, that group intelligence test *scores* increase with the amount of education. Moreover, he has been able to demonstrate from his data that when boys are roughly matched for mental ability at age 14, subsequent education is effective in increasing the group intelligence test scores of the better trained.

I have no quarrel with this last conclusion, although I think the effect of education is somewhat less than Lorge represents it to be. I do strongly object, however, to his use—or misuse—of the term "IQ." The uncritical impression gained from Lorge's paper, namely, that "education raises the IQ," has been aired in the newspapers, and is already being parroted by those who would like to believe that the school book is more powerful than original nature. Unfortunately, the correction of a wrong notion often requires the presentation of technical detail which is, in general, carefully avoided by the loquacious but untrained reader. As a result, a popular fallacy, once established, is extremely hard to eradicate. The present instance offers a good illustration of this fact, and is another reason why the scientific writer should be extremely careful what he puts into print.

There are two conclusions to be drawn from Lorge's report—the one true, the other false. I shall state these conclusions in order and summarize briefly the evidence for each.

*Conclusion I; The more recent and the more extensive a person's education, the better he is likely to perform on tests requiring reading, information, vocabulary, arithmetic—i.e., on tests involving words and numbers.*

This general result has been known for a long time; and any other finding would certainly seem quite unlikely. I shall cite evidence from only one source:—the extensive data furnished by the Army Alpha test, a verbal examination administered to nearly two million men in 1917-18. The correlation<sup>2</sup> between extent of schooling and Alpha

<sup>1</sup> LORGE IRVING, Schooling Makes a Difference, *Teachers College Record*, 46: 1945. 483-92.

<sup>2</sup> YERKES, R. M., Ed., *Memoirs National Acad. of Sciences*, 1921, 15, 748.



score in a group of 48,102 recruits was .74. Among white enlisted men, scores in Alpha increased steadily with education: for men who had completed 5-8 grades, the mean was 51.1; for men who had completed high school, 92.1; for men who had completed college, 117.8. Scores made by officers declined steadily with age (recency of schooling); clerks (workers with words and numbers) scored higher than machinists (workers with tools); chaplains higher than dentists.

The evidence for the influence of education upon verbal intelligence tests seems, then, to be clear and convincing. Lest the educator become too complacent, however, I mention in passing that officers who reported schooling of 4 grades or *less* scored 20 points higher on Alpha (112.5) than white enlisted men who had completed high school (92.1). Perhaps there is food for thought in the suggestion that even a high school education is not always a substitute for brains in the first instance.

Lorge's data warrant special consideration since he has studied the effects of schooling upon test scores of men who 20 years earlier were equated (at least roughly) in group intelligence test score. It will be helpful to review briefly Lorge's experimental material in order to provide a proper setting for subsequent discussion. In 1921, 863 boys around 14 years of age were given a series of abstract intelligence, mechanical aptitude and clerical ability tests. In 1941—20 years later—131 of the original 863 were located and induced to take the *Otis Self-Administering Test, Higher Examination, Form B* (time limit: 20 minutes), and *Part III of the Thorndike Intelligence Examination for High School Graduates, Form V* (time limit: one hour). According to Lorge, this sample of 131 was not significantly different in mean achievement or in variability from the original 863, and hence was fairly representative of the larger group. The educational achievement (highest grade completed) of these 131 survivors varied from 8 grades to 17 or more (beyond college), and the opportunity is afforded, therefore, to discover how differences in extent of schooling affected the 1941 scores of boys equated for test score in 1921. The 131 "boys"—34 years old in 1941—were classified into six groups on the basis of their 1921 group intelligence test scores; and 1941 scores on Otis SA were tabulated for various levels of academic achievement.

Table I reproduces Lorge's Table I, and Table II is my summary of certain of these data. Consider the scores in Table I, column 89-98, the data especially cited by Lorge. A total of 23 boys out of 131 scored from 89 to 98 on the 1921 test. Of these 23 the 2 who completed grade 8 had a mean score of 39.0 on the 1941 Otis test; the 2 who completed

TABLE I\*

AVERAGE SCORES ON THE OTIS SELF-ADMINISTERING TEST OF MENTAL ABILITY, HIGHER EXAMINATION, FORM B (20-MINUTE TIME LIMIT), TAKEN IN MAY 1941, CLASSIFIED BY SCORE ON THE 1921-22 TEST OF ABSTRACT INTELLIGENCE AND BY HIGHEST GRADE COMPLETED BY MAY 1941: FOR 131 BOYS WHO MAY BE CONSIDERED A REPRESENTATIVE SAMPLE OF THE VOCATIONAL GUIDANCE GRADE GROUP

Highest Grade Completed by 1941	Score on 1921-22 Test of Abstract Intelligence											
	49-58		59-68		69-78		79-88		89-98		99-114	
	Otis		Otis		Otis		Otis		Otis		Otis	
	Score	N	Score	N	Score	N	Score	N	Score	N	Score	N
8	14.0	4	22.0	4	20.7	9	26.4	5	39.0	2	33.0	1
9	19.0	1	19.5	2	14.5	2	31.1	8	38.0	2	29.0	1
10	24.0	1	22.0	4	25.1	9	28.5	8	37.0	4	46.5	2
11 or 12	21.0	1	26.0	1	31.7	3	31.0	9	41.0	3	34.0	1
13 or 14			22.0	1	26.0	3	34.7	4	41.7	4	37.5	2
15 or 16			34.0	1	27.0	1	39.5	6	53.5	2	50.8	5
17 or more					38.0	3	46.0	5	54.5	6	43.0	1

TABLE II

## SUMMARY OF CERTAIN DATA FROM TABLE I

Highest Grade Completed by 1941	Intelligence Test Scores: 1921-22									
	56-68		69-78		79-88		89-98		99-114	
	Otis		Otis		Otis		Otis		Otis	
	Score	N	Score	N	Score	N	Score	N	Score	N
8- 9-10	21.5	10	22.1	20	29.0	21	37.8	8	38.8	4
13-16	28.0	2								
15-17 plus			35.3	4	42.5	11	54.3	8	49.5	6
Difference:	6.5	12	13.2	24	13.5	32	16.5	16	10.7	10

\* From Lorge, *op. cit.*, p. 487.

grade 9 had a mean score of 38.0 on the 1941 test, and so on. The 6 boys who completed 17 or more grades (college and beyond) had a mean 1941 score of 54.5. Averaging the three entries at the top and the two entries at the bottom of this column we find (Table II, column 89-98) that the 8 boys who completed 8, 9 and 10 grades had a mean 1941 score of 37.8, while the 8 boys who completed 15 or more grades had an average 1941 score of 54.3. These two groups differed by 16.5 points on Otis SA in 1941, and this difference Lorge attributes to the better schooling of the second group. Several comments are in order:

1. The 16 boys in column 89-98, Table II, were not *strictly* equated in score in 1921. Their scores, in fact, show a 10 point range (89-98), while in 1941 the range between the 8 with the best and the 8 with the poorest education is 16.5 points. These ranges, to be sure, are not directly comparable; the tests are in different units and the second range is between two selected means and is restricted as compared with

the original range of 10 points. It seems reasonable to infer, however, that had the 16 boys been more exactly equated in 1921 score, the gain of 16 points might have been somewhat reduced. Evidence for this inference can be found in the other columns of Table II:—differences in Otis SA score attributable to 7–8 years of schooling are 6.5, 13.2, 13.5, and 10.7. Lorge, incidentally, gives comparative data only for the column in which the change is greatest.

2. It seems clear that 7–8 years of additional schooling *did* make a difference in the performance of these 8 subjects upon group intelligence tests. But the change wrought by education is certainly modest and hardly warrants a feeling of smugness on the part of their teachers.

3. Any conclusion as to the effects of schooling upon the Otis test scores of subjects equated for intelligence level 20 years earlier must necessarily be qualified by the small sizes of the samples. The 16 boys cited above bear a heavy burden; and results from even 131 subjects can at best be regarded as suggestive rather than conclusive.

*Conclusion II: "Schooling raises the IQ." There is no evidence in Lorge's report to substantiate this statement.*

We have seen that the 8 boys who scored between 89 and 98 upon an intelligence test in 1921, and whose subsequent schooling was 8–9–10 grades, had an average score of 37.8 on the Otis SA in 1941; and that the 8 boys who scored between 89 and 98 upon an intelligence test in 1921, and whose subsequent schooling was approximately 4 years of college, had an average score of 54.3 on the Otis SA in 1941. According to tables supplied for the Otis SA examination, a score of 38 is *equivalent* to a "Binet IQ" of 103, and a score of 54 to a "Binet IQ" of 115. The first score, namely, 38, is also found from the tables to be *equivalent* to a "mental age" of 16–5; the second score of 54 to a "mental age" of 18–5. Comparison of these two MA's and two IQ's furnishes the basis for Lorge's statement that for boys of "equal" intelligence at age 14, 7–8 years of schooling produce an increase by age 34 of "two full years" in MA (i.e., 16–5 to 18–5), or a change in IQ from 103 to 115.

It is worthwhile examining further this entirely erroneous conclusion. Psychologists conversant with test construction know that rarely if ever is an MA or IQ derived from a group test even remotely comparable to the IQ achieved on Stanford-Binet. This is recognized by Otis himself, who writes<sup>2</sup> "The term *Mental Age* (capitalized), however has now come to have a special meaning and to denote measures of mental ability—i.e., scores—in the Binet-Simon tests. Binet mental

<sup>2</sup> OTIS, A. S., *Manual of Directions, Otis Self-Administering Tests of Mental Ability*, Yonkers, World Book, 1928, p. 4.

ages below about 13 years are true mental ages. Above that, especially above 16 years, they are *merely scores*." (Italics mine) Again, Otis writes:<sup>4</sup> "... IQ's as sometimes derived from group tests of mental ability bear little relation to IQ's derived from the Binet tests." To be sure, Otis has attempted by means of an ingenious chart to provide MA's and IQ's which are *estimates* of Binet MA's and IQ's that subjects *might* have earned when younger. But such estimated MA's and IQ's are neither psychologically nor statistically equivalent to Binet values, and the psychologist who uses these terms without proper explanation and qualification—as does Lorge—performs no service other than that of confusing and misleading his readers.

The IQ is a developmental concept, suitable for use with age-scales such as Stanford-Binet. The function of the IQ is to balance mental status against life age over the period when growth is progressing. Up to age 14-15 the IQ is useful and informative; after this period the MA no longer increases with CA and the IQ is of no value. To use the term IQ with reference to 34 year olds is indefensible and incorrect.

The IQ should be used only with tests so constructed that MA's derived from test scores will, when divided by CA, return a constant ratio. Requirements for a constant IQ are well known<sup>5</sup> but perhaps may be profitably repeated here. In order to yield a constant IQ a mental test must (1) exhibit IQ distributions, the *SD*'s of which are equal at each age level (this means that *SD*'s of MA distributions must increase systematically with age); (2) measure the same abilities at successive age levels; (3) provide IQ's which show no consistent tendency either to increase or to decrease with CA.

The requirements for a constant IQ are *not* met by any group tests with which I am familiar. They are met by the Stanford-Binet. The term IQ, therefore, should *never* be used with group tests but should be confined strictly to tests of the Binet type. To be sure, some group tests<sup>6</sup> keep the IQ approximately constant by assigning an IQ of 116, say, to the score in each age distribution one sigma above the mean; an IQ of 132 to the score two *SD* above the mean, etc. But such statistical IQ's are *not equivalent* to Stanford-Binet IQ's, and when used interchangeably confuse rather than inform. The Otis SA Test does not provide MA's and IQ's equivalent in meaning to the Binet values. Lorge's use of the terms MA and IQ, therefore, is unwarranted. And the implication from his study that 7-8 years of education will *raise* the IQ from 103 to 115 ("two full years") is without foundation in fact.

<sup>4</sup> OTIS, A. S., *op. cit.*, p. 5.

<sup>5</sup> McNEMAR, QUINN. *The revision of the Stanford-Binet Scale*. New York: Houghton Mifflin, 1942, p. 155.

<sup>6</sup> *Pintner General Ability Tests, Verbal Series*. Yonkers, N.Y.: World Book, 1939.



## NOTE ON DUNLAP'S REMEDY FOR DEFECTIVE COLOR VISION

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It is a well-known fact that normal color vision is a requirement for qualification in certain industrial occupations. Because of the occurrence of some degree of defect in color vision in five to ten per cent of the male population, there has been a great incentive during the war years to discover some means by which otherwise eligible men in this group could be salvaged for military specialties. Diligent research sponsored by Army and Navy experts has failed to produce favorable evidence for any remedy for color blindness.

Among the various remedies tested was vitamin A. It appears that the credit for suggesting vitamin A for this purpose belongs to Dunlap and Loken (1). The general effect of the suggestion, in addition to stimulating some unsuccessful experimental attempts to confirm it, was to encourage the development of quick cures for men who wished to evade the draft by volunteering for officer training. For a time the number of such treatments constituted a small racket which was not easily curbed. (2)

While the problem of correcting defective color vision seems less urgent with the end of the war, the fact that Dunlap (3) persists in an elaborate defense of his original unsubstantiated notions on the effectiveness of vitamin A requires some examination.

In reading Dunlap's latest report one is reminded constantly of the difficulties of conducting research from an academic base on a shifting wartime population. The incidence of frustrated plans; the prevaricating, vagrant, and otherwise unreliable subjects; and the sudden induction of men on the threshold of a cure—all tend to make the reader of the report sympathetic. Under such difficulties it is not surprising that the results obtained were of little value.

Aside from some original and very novel speculations as to the nature, distribution, and possible causes of color blindness the only new material presented by Dunlap consists of 14 case histories of individuals who received vitamin A and cobra venom. In some cases perhaps vitamin B was given. The criterion tests were Loken's modification of the Nela test, and the Ishihara and American Optical Company pseudo-isochromatic plates.

In 13 of the 14 cases presented the treatment was not complete, according to the author's own statement. In the other case the conclusion was that the defect was irremediable. If the summary state-

ments given on each case may be interpreted literally, then Dunlap himself rejects 12 of the 14 cases as questionable. On the basis of results presented on tests before and after treatment he should have rejected 14 out of 14.

To fully appreciate the complete failure of these 14 cases to give support for vitamin A treatment it would be necessary to examine the data and to comment on each one, but detailed review here is not justified. Of the group as a whole Dunlap says: "While no conclusions could be drawn from these cases, their features suggest certain probabilities and agree with some of the notions which had occurred to us earlier."

As a check on Dunlap's original report that 80 per cent of his cases were able to pass chart tests after taking vitamin A, Elder (4) examined approximately 900 R.O.T.C. cadets at Louisiana State University. Of the 65 men showing various degrees of defective color vision, 41 completed the course of treatment. Results were entirely negative. The method used in this experiment closely paralleled that of Dunlap's even to the use of Ishihara and American Optical Company charts which he now condemns as unreliable but continues to use.

Dunlap suggests that pro-vitamins were used in the LSU experiment and that the units of vitamin A administered were unknown, presumably because the material was obtained from a questionable source. A careful reading of my report shows that "the material used was a vitamin A ester of high potency, determined spectrographically and confirmed by bio-assay" and that it was supplied by the Norwich Pharmacal Company. The implication that vitamin products of this company may be inferior to those of Squibb, Lilly, or Upjohn, indicates that Dunlap is unaware of Norwich research facilities and its systematic assay of all vitamin preparations.

If Dunlap persists in defending the notion that vitamin A has a beneficial effect on defective color vision, we are entitled to request that, in his next study, he observe the following conditions:

a. Use only subjects with demonstrable defect (by any standard criterion he chooses).

b. Leave nothing to be presumed as to whether subjects take their doses regularly and in prescribed amounts. Supervised administration is not difficult.

c. Carry through *complete* treatment on at least a few cases so that there will be no need for guessing as to what might have happened if doses could have been continued for a few more weeks.

d. Take necessary precautions against the possibility of learning the tests. (It is known that many men have passed service tests in this way. Confirmation of final test results with an additional test which the subject had not seen previously is desirable.)

It is impossible to abandon completely the hope that Dunlap's notions may lead to new knowledge of treatment for defective color vision. Until we have that knowledge in the form of thoroughly reliable observations, further discussion is superfluous and misleading.

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## BOOK REVIEWS

LINTON, RALPH (Ed.) *The science of man in the world crisis*. New York: Columbia Univ. Press, 1945. Pp. xiv+532.

The science of man referred to in the title of this symposium is anthropology. The editor, himself an anthropologist, feels that the time is ripe for a synthesis of the sciences which deal with human beings and their problems. Though "anthropology is by no means the only discipline which has concerned itself with the study of man," he maintains that it holds a central position among these disciplines and that it should assume the responsibility for synthesizing their contributions.

The purpose of the particular synthesis attempted in this volume is to give people who are planning a more satisfactory world order a better understanding of the potentialities and limitations of the human material with which they are dealing and to provide them with techniques for controlling that material. Several chapters deal in a practical and helpful way with world resources, world population problems, and the administration of Indian and colonial affairs. One chapter shows how the results of the analyses of American radio audiences and programs might be helpful to a world government in its attempts at influencing public opinion.

Though there are several references to the need for a study of the biological man, no biologist is included among the 22 contributors. This is the more surprising since the anthropologists seem agreed that culture has no existence apart from the individuals who originate, transmit, and change it, and, therefore, that culture must bear the imprint of human nature.

Klineberg is the only psychologist contributing to this volume. His chapter on racial psychology is an excellent, carefully worded, brief survey and interpretation of psychological research in the area of racial differences.

In their discussions of cultural assimilation and change, several of the anthropologists lean heavily on learning theory as developed by the Yale Institute of Human Relations. Cultural changes are explained in terms of their capacity to relieve anxieties or to provide positive satisfactions. On the other hand, it is evident that some anthropologists do not find this theory adequate to explain all aspects of cultural transmission. The child seems to acquire some of the culture of his society in an automatic fashion in which it is difficult to discern any signs of either drive or reward.

One chapter reports a symposium on the concept of culture. The participants are several anthropologists, a psychologist, an historian, a lawyer, a physician, a psychiatrist, a philosopher, a business man, and an economist. Psychologists will find it revealing and chastening to get the anthropologist's reactions to their theories, approaches, and terminology.



It is evident from this book that modern anthropology is increasingly interested in the universals of human nature and of human cultures. One chapter, on the common denominators of culture, deals entirely with the surprisingly large number of activities, such as religion, faith healing, dancing, family living, incest taboos, fire making, and cooking, which are found in all cultures, past or present, throughout the world. Instead of attributing these universals in a facile fashion to instincts or even to simple biological needs, the author shows in a clear and penetrating fashion how they are the outcome of the complex interplay of these needs with secondary, acquired motives developed as a result of the world wide functioning of the principles of learning.

This book marks a welcome, albeit modest, advance toward an actual synthesis of the sciences which study man. Its principal value to psychologists will probably be (1) as evidence of the values to be derived from closer cooperation with the other biological sciences and (2) as a survey of the frontiers of research in anthropology proper.

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*Antioch College.*

KARDINER, ABRAM, (with the collaboration of) LINTON, RALPH, DU BOIS, CORA, and WEST, JAMES. *The psychological frontiers of society*. New York: Columbia Univ. Press, 1945. Pp. xxiv+475.

Rarely are psychologists presented with a book more challenging in its social implications than this one. It is truly a cooperative endeavor, involving not only the skill of the authors named in the title above but also of Dr. Emil Oberholzer who analyzed independently, or without collusion, 38 Rorschach tests which had been administered by Du Bois to natives of Alor. The raw data were supplied by the collaborators. They consist of careful descriptions of three cultures, namely, the Comanche culture as described by Linton, the Alorese culture pictured by Du Bois, and the culture of an American rural community presented by West. Although the analyses and interpretations of these cultures, as well as the conclusions, are the work of Kardiner, there is evidence indicating a frequent interchange of ideas among all of the collaborators with the exception of the Rorschach specialist. The result is a well-coordinated book in spite of the diversity of its subject matter.

The concept which is indispensable for the interpretation of the different societies is *basic personality*. There can be no doubt that Linton in his *Foreword* and Kardiner in Chapter II, *The Technique of Psychodynamic Analysis*, but more especially in the main body of the book, have indicated the anatomy and dynamics of basic personality with greater clarity than they achieved in an earlier volume entitled, *The Individual and His Society*. Preferred to all others in deriving the characteristics of basic personality is the psychoanalytic technique.

Not, however, basic drives or "instincts" but action systems with identifiable perceptual, coordinative, and executive features are the fundamental constituents of basic personality. There is a generous departure from Freudian theory, a departure dictated somewhat by the anthropological data. It must not be assumed that Kardiner has approached his task with ready-made concepts, or that he has forced the data into preconceived molds. The nuclear constellations of basic personality, differing as they do from society to society, have been distilled from materials collected by the anthropologists. Kardiner, therefore, has not merely repeated the anthropological findings to append to them facile interpretations, rather he has painstakingly studied the data to derive his concepts from them and to uncover relationships not readily discernible to the original investigators. He has shown, for instance, how in infancy and early childhood the impact of maternal care, sibling interactions, paternal dominance or its absence, and of conduct more indirectly related to institutions, establish action systems which in their dynamic interactions yield basic personality. These action systems give rise to projective systems which later determine, for example, religion, folklore, and the arts. The projective systems are supplemented by reality systems which deal with explanations of the outer world and ways of dealing with it, or with conventionalized means of getting along with other people. Projective systems and reality systems together constitute basic personality but the former are both primary and dominant; in fact, the projective systems exercise a directing influence over the reality systems.

Kardiner does not stop with the derivation of the type of basic personality characterizing the three cultures studied intensively, instead he proceeds to demonstrate how the basic personality, in turn, explains many of the differences among the cultures and within each society. He has sought, in a measure, to deal also with the problem of variability among the members of a given society, a problem which was neglected in the earlier volume referred to above. In this task he derives considerable aid from biographies of persons in Alor and in *Plainville, U.S.A.*

The similarity of interpretation between analyses of personality made by Kardiner and by Oberholzer, who did the Rorschachs "blindly," is striking. At least this is so for what Kardiner calls the diagnostic features but not for the dynamics of behavior. Even though we are told there is no substitute for actually living with people, the correspondence in interpretation leaves us wondering which method of analysis contributes more to the validity of the other. Both methods combined suggest problems not likely to have been discovered by either one alone.

In the final or fourteenth chapter, Kardiner permits himself a flight of controlled imagination, a thought provoking and intriguing in-

terpretation of history in accordance with his concepts of basic personality. His discussion of the social significance of projective systems and the part they play in stabilizing the social order is packed with meaning for the psychologist and sociologist.

Only in the first chapter does the author appear to betray a slight aggressiveness not accounted for by the psychological techniques he condemns. Classical psychologies generally associated with the names of Wundt, Watson, Pavlov, and of Wertheimer, Köhler, and Koffka, are criticized because they render little or no aid in determining the dynamics of social behavior. Lewin's topology is dismissed in three sentences, or shall we say it is identified with psychoanalysis as an experimental handmaiden? Yet Kardiner throughout the book does not hesitate to write of circumstances conditioning the child one way rather than another. Had he desired to trace carefully and specifically how the earliest components of action systems were learned and integrated, I suspect his indebtedness to Pavlov would not be less than that owed to Freud. Psychoanalytic methods are not applicable to all aspects of experience and behavior, nor are the methods of any one of the current psychologies Kardiner has found wanting. Since psychoanalytic techniques and concepts enabled him to explain even the apparently unrelated forms of behavior, institutionalized and otherwise, he did not need to defend his preferred psychological system through a series of counter-attacks.

The book is not a text for undergraduate students, although the anthropological accounts will have an intrinsic appeal to them. A considerable knowledge of psychodynamics is required to follow and understand the analyses of the several societies. Graduate students in psychology, sociology, and anthropology, and especially their instructors, have been challenged to reexamine their basic concepts and interpretations of social behavior. They cannot afford to neglect a volume as richly suggestive as this one.

CHARLES BIRD.

*University of Minnesota.*

LAZARSFELD, P. F., BERELSON, B., & GAUDET, H. *The people's choice. How the voter makes up his mind in a presidential campaign.* New York: Duell, Sloan, & Pearce, 1944. Pp. vi+177.

This book is not a report of a routine political public opinion poll but is a cogent analysis of the factors that influence the voter in making his decision. As such, a careful reading will repay sociologists and political scientists as well as psychologists.

The first two sections lay the background by describing the people and social milieu of Erie County, Ohio, chosen as the sampling area because of its relative representativeness. Two sections are devoted to a discussion and interpretation of the data. Appendices refer the reader

to appropriate source material and elaborate certain technical items.

The data were gathered by eight waves of personal interviews obtained at one-month intervals just prior to the 1940 presidential election. Three control groups were used to determine whether the initial interviews with the main panel would make them "election conscious" and influence responses on subsequent questionnaires. Intensive repeat interviewing permitted a thorough study of voters' characteristics, but more important, allowed respondents' attitudes and opinions to be related to events of political significance soon after they occurred.

Social and cultural factors are demonstrated to be marked determinants of voters' preferences, and although people made up their minds to vote in a given way at different stages of the campaign, they tended to do so in terms of their political predispositions. The political campaign developed interest in the election for the indifferent, but did little converting of the partisan. Campaign arguments merely provided the latter with rationalizations to support his preferences. "Opinion leaders," who were more alert to events but more unresponsive to arguments opposing their predilections, were found to affect the mass of voters more than formal discussions in magazines and newspapers and on the radio.

The information presented regarding election behavior is in itself important, but in their ability to evoke meaning from the data, the authors made their greatest contribution. Fractionation and recombination of the data bring out pertinent relationships that would not be apparent from a simple tabulation of responses. Some will regret that the methods employed in gathering the data were not treated in greater detail, but few will deny that the use of scientific polling techniques as demonstrated in this book will make possible the addition of new concepts in social psychology.

LESTER GUEST.

*The Pennsylvania State College.*

LINDNER, R. M. *Rebel without a cause; the hypnoanalysis of a criminal psychopath.* New York: Grune & Stratton, 1944. Pp. ix+296.

For those who have never had first hand experience with psychoanalysis—nor read James T. Farrell—this book will be both interesting and educative. The suggestion of Sheldon and E. T. Glueck in their short introduction that the book is a significant milestone in criminology, however, is rather startling in view of the fact that it presents the results of only one case and uses a method that is by no means new in spite of the author's seeming claims in that direction.

Two hundred and fifty-nine of the 289 pages of text are the actual stenographic results of the psychoanalysis of Harold, who is psychiatrically diagnosed as a psychopathic personality and who is heavily



sentenced to the penitentiary for a serious crime, the nature of which is not disclosed. During the analysis Dr. Lindner meets the customary resistance from his subject and speeds its resolution with the aid of hypnotic recall, justifiably pointing out that this valuable therapeutic technique has been too cavalierly dismissed by analysts ever since The Master discovered it was not a panacea.

The conclusion of the author that his method has verified "the major but until now unproven hypotheses of the traditional psychoanalytic view of this entity" (psychopathic personality) seems presumptuous in view of the fact that (1) he has not been dealing with an entity, and (2) the logical meaning of proof is something more than restatement on the basis of one case. Yet the fact that hypnoanalysis as a method of diagnosis and treatment has great value is overwhelmingly evident even when the actual results are down in black and white and therefore robbed of some of their emotional force. That Dr. Lindner has skillfully conducted the treatment is also apparent, yet it will be this necessary skill combined with an admittedly rare *rapproch* which will limit the widespread acceptance of this method for practical penological uses.

The first and second chapters discuss respectively the problem of criminal psychopathy and the author's method employed in its study. With the exception of a short summary chapter the remainder of the book is given over to the analysis record with occasional interpolated remarks by the author.

With a brilliant stroke of the technical *mot juste*, the author describes the definitive and classificatory role of psychopathic personality in the psychiatry of the past as a Pandora's Box. The force of the comparison for critical purposes, unfortunately, is somewhat clouded in his succeeding discussion when the psychopathic personality quickly becomes a "they" to which are attributed both general and specific typing characteristics. It is indeed strange to read on the first page of chapter one, for example, that this catch-all category has no accepted delimitation, and then on the last page of chapter two that the author's method has finally "penetrated to the core of psychopathic personality for the first time in the long history of psychological concern with this puzzling classification." Nowhere in the pages between or the ones that follow is there presented any definition of this "entity" which would give it a core to be penetrated.

The situation is further clouded by the facile use of analytic circularisms which, as usual, pose as explanations. Although the author recognizes that the statement: "The psychopathic personality super-ego is weak" is circular (he uses the words "self-explanatory"), he feels no dissatisfaction when he explains the acknowledged infantile behavior of the psychopath as "an abrupt cessation of psychosexual development." In addition to the possible multiplication of such logical

sterilities, the author can also be charged with illegitimate borrowing of what may well be principles of physiology but are certainly fictions at the level of total behavior. Cannon's canon of homeostasis is indeed well illustrated by the intricate nervous regulation of man's internal environment and in this connection serves as a big gun, but it is hardly permissible to say that the adjustive behavior of the neurotic is comparable to the range in *pH* content of the blood. Nor is it permissible, to the reviewer at any rate, to conclude even speculatively in favor of a structurally defective brain in psychopathic personality because the behavior of the latter is without restraint and the supposed function of higher nervous centers is to exercise restraint over lower centers.

The problem of the legitimacy of infantile memories is solved to the author's apparent satisfaction by comparing the motor reactions of his subject during hypnotic recall with the Gesell norms for child development. Harold's crucial six to eight months' memories are accepted as valid, but the author does not report whether or not at this time his subject was putting his own foot in his mouth.

G. RAYMOND STONE.

Indiana University.

ENGLE, T. L. *Psychology: principles and applications*. New York: World Book Co., 1945. Pp. ix+549.

This textbook was written for high school students who will probably take no further formal courses in psychology. It is a self-contained presentation and omits bibliographic references to other volumes or experimental literature. Accompanying the text is a *Teacher's Manual* containing some of these references and other material to facilitate the subject presentation. The author had extensive experience as a teacher of high school psychology before moving to the college level.

Throughout the text, the author prepares a factual and attitudinal foundation for students who will continue their exposure to psychology only through such "advanced 'texts' . . . (as) magazine articles, radio programs, motion pictures, sermons, and lectures; . . . (while) their psychological 'laboratories' will be the homes, the businesses, the clubs, and the communities in which they will function as citizens."

Most of the 17 chapters present subject matter of immediate interest and practical value to high school students. The following sample chapter headings illustrate the functional style which keynotes the entire volume: *Friendship and Love*; *Popularity and Leadership*; *Improving Learning Techniques*; *Getting in Touch with Our Environment*; *The Senses*; *Unusual Personalities*; and the final apt chapter heading, *Concerning Several "Mysterious" Matters* (hypnosis, dreams, etc.). However, there are three chapters which are less consistent with this central theme. Chapter I, *An Introduction to the Science of Psychology*, includes a discussion of the scientific method and a short statement of

the special fields of psychology. This material is, by necessity, highly abstract and would probably be more meaningful after the student has become familiar with some specific problems and procedures of psychological analysis. Chapter II, *Preparing to Read Psychology*, is a summary of basic statistical methods. The same criticism applies here particularly since far less use is made of statistical terms in the remaining chapters than is indicated by the emphasis given in this chapter. Both of these chapters have considerably less inherent interest to the high school student than the information in the rest of the book and it is unfortunate that this traditional, though questionable, introduction should be presented to students of this level.

The second of two chapters on learning is better than the first since it is an application of fact and theory to the learning-in-school situation while the first is a digest of the basic concept and experimental techniques used in both animal and human studies. Also included in the first chapter on learning is a well written discussion of thinking. This section might profitably have been expanded into a separate chapter.

This must have been a difficult text to write. Undoubtedly it is easier to list in summary form a series of related psychological studies than to interpret these findings in a way that will answer fundamental psychological questions confronting high school students. Many times the author must have wished that psychologists would or could find ways of examining the psychological attributes of more daily bread and butter problems. However, rather than be tempted into a series of student-interest lessons of psychology applied to daily life, the author consistently presents and interprets information drawn from scientific and critical source material.

This book includes a large number of illustrations and examples drawn from contemporary events—particularly material resulting from psychological and related scientific contributions developed during the war. As this information becomes more widely disseminated and continues to be released from military restrictions, the volume will acquire even greater practical applicability for high school students and teachers.

The reviewer recommends this textbook for examination by every teacher of high school psychology. This book should aid in making available to a large number of younger students the more practical results of psychological research and analysis.

STANFORD C. ERICKSEN.

*University of Arkansas.*

BIRD, CHARLES & BIRD, DOROTHY M. *Learning more by effective study.* New York: D. Appleton-Century, 1945. Pp. viii+275.

This is another book on how to study. The fact that more than forty books and manuals on this subject have been published in the United

States since 1926 is indicative of the importance of the problem treated. It may also signify that individuals working in this field have not been entirely satisfied with the productions of their colleagues and co-workers. A major weakness of many of the available texts on how to study is that their numerous suggestions have been based largely on opinion rather than upon research in the field or upon sound principles of the psychology of learning.

This new book by the Birds, however, is refreshingly different in this respect. The procedures for effective study which they advocate all appear to be founded upon well validated psychological principles and theories which have come from the experimental literature. Instead of giving numerous conflicting and confusing rules for study, this book suggests a few highly significant methods and principles which have wide applicability. The authors, furthermore, appear to be concerned with the total personal adjustment of the student as well as with his academic achievements.

The book is not only psychologically sound in its approach, but it is conservative and accurate in its claims as to the amount of improvement that can be expected to result from special attempts to develop better study habits. For example, in discussing the progress made by college students who devoted eight weeks to the improvement of their speed of reading, the authors state that "they read 16.5 per cent faster in the final than in the initial test." (99) Although this finding is undoubtedly typical of the improvement one might expect to find in this particular skill during such a period of practice, it is quite unusual to find authors who do not make much more sweeping and extravagant claims.

On the whole, the book *Learning More by Effective Study* represents a very useful contribution to our knowledge of the psychology of study. College students, high-school seniors, and adults who will carefully follow its suggestions can with confidence expect to make substantial improvements in their study techniques.

GLENN M. BLAIR.

*University of Illinois.*

ABRAHAMSEN, DAVID *Men, mind and power*. New York: Columbia Univ. Press, 1945. Pp. viii+155.

A psychiatrist and refugee from Norway holds psychoanalytic post-mortems on the German nation, Hitler, Goering, Goebbels, Himmler, Quisling and Laval.

The people of Germany are diagnosed somewhat in Adlerian fashion as chronically suffering from feeling of inferiority with pronounced compensatory strivings for recognition. Their humiliation from defeat in World War I touched such a new low that temporarily they were at a complete loss for a way of compensation. Into this situation stepped



those arch-antinomians, Hitler, Goering, Goebbels and Himmler, acute sufferers from long-standing personal inferiority-feeling who thought they saw at once for themselves and the nation a way of salvation—a way to a thousand years of *pax Germanica*—in a projection of the causes for German misery upon the Jews, Communists and capitalistic nations and in a ruthless destruction or subjugation of these imaginary enemies. The Aryan population of Germany became as one in enthusiastic acceptance of this highly neurotic fiction which alone gave promise of a restoration of status and which was to eventuate in so much world-havoc. In anti-German countries, particularly France and Norway, elements of the population like Quisling, Laval et al., long smarting from frustration and non-appreciation, saw likewise a certain salvation for themselves, an opportunity for recognition in a throwing of their sympathies and activities with Hitler's new order against the interests of their own countrymen.

The latter part of the book is devoted to psychotherapeutic measures which Abrahamsen thinks should be taken by the United Nations in the interest of a permanent cure for this grand status-neurosis of the German people. Fully cognizant of the delicacy with which treatment must be undertaken by outsiders if the trouble is not to be exacerbated, the author indicates among others two rather significant approaches: 1. the replacement of men teachers in the schools by wholesome minded women; 2. a deführerizing of the father, i.e. a democratization of the German family set up.

The nuclear ideas of this book appear of interest to the general reader and most certainly provide food for thought for criminal and social psychologists. Unfortunately these ideas are somewhat obscured by the author's style which might be described as semi-free association.

F. C. SUMNER.

Howard University.

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